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Addresses delivered at the Annual Meeting of the British Association for the Advancement of Science in Sheffield, August 29 to September 5, 1956

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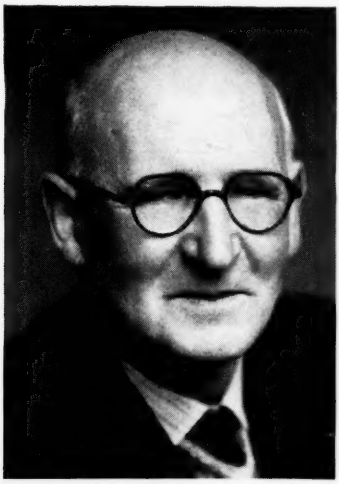

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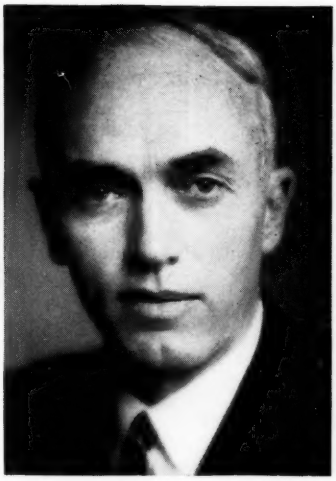

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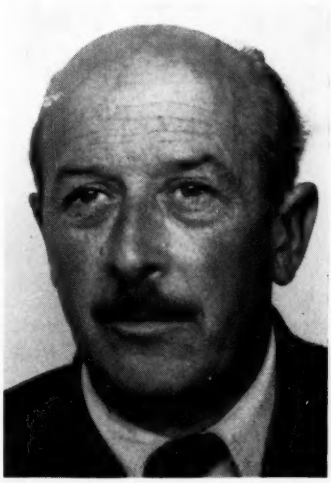

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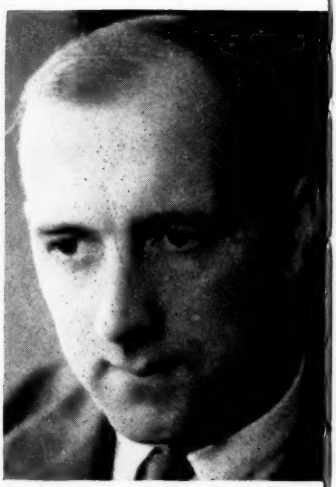

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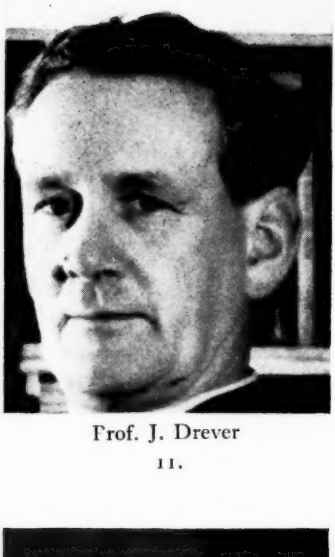

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TWENTIETH-CENTURY MAN AGAINST ANTARCTICA

ADDRESS BY SIR RAYMOND PRIESTLEY, M.C.

PRESIDENT OF THE ASSOCIATION

THE British more than most peoples go in for paradoxes. It is therefore not atypical that the British Association should, once in a while, elect as its President a man who has been away from science for some forty years. After all, even in this technological age, the value and the advancement of science in a democracy both depend to a considerable extent, upon a correct appreciation of the importance of science by the layman at large. There was an element of statesmanship in the man who coined the phrase 'we must educate our Masters'. We cannot afford to fumble over this vital question of education while we exist precariously in a competitive world. Especially is this so when at least one of our rivals is not yet committed to majority rule, but has her policy directed by men who have both the sense to mobilise the wealth of new knowledge technology is placing at man's disposal, and the power to compel the rank and file to use that know-how to the best advantage. So I shall make no apology this week as I wander from Section to Section as—shall we say—a touchstone of intelligibility.

As I stand here in my present predicament, I draw courage from two facts. The British Association is this year undertaking a serious study of how it can best discharge its particular responsibility in the circumstances of today, which are as different from those that prevailed when the Association was founded over a century ago as chalk is from cheese. The Association's functions of integration and dissemination are expressed in its title and set out at greater length in its constitution. Today they are more important than they ever were before, and the duties involved are much more difficult to perform. The world can no longer afford to wait until an annual convention for the announcement of major discoveries in science. A great apparatus of scientific periodicals has in any case removed the need. Yet the annual parade of science before the people should have its value. At that parade science must wear the abbreviated bathing-costume of the nineteen-fifties rather than the Victorian crinoline, so that the beauty, the challenge and the danger of the truth may be seen by all. This aim can only be achieved to the extent that the scientist can state his results in language that can be appreciated at least by the average educated citizen. An approach towards that ideal is one task that we have given to our Division for the Social and International Relations of Science.

A second task that must follow from this first is to attract endowment sufficient to make our means—adequate enough a hundred years ago when pounds were pounds—equal to the performance of a greater task in a more expensive world. To solve the latter problem we must do two things. We have to make our meetings so attractive that our membership taxes the accommodation of the great cities in which we meet. At the same time we must convince industry that we have a task worth doing and are equal to that task. This should not be beyond our powers if we think our first problem through.

My second comfort tonight is the fact that I have chosen for my main theme a subject about which, if I know too little, the great majority of my listeners will from direct experience at any rate, know less. Antarctica is remote. It has therefore still to a great extent the fascination of the unknown. It is vast. Its area is six million square miles; nearly as large as Europe and Australia together. The greater part

of the continent's surface lies between 6,000 and 11,000 feet above sea-level and is covered with ice several thousand feet thick. The oceans surrounding it teem with life as do no other waters in the world. For half a century whaling man has followed in the explorer's track to farm these seas. The continent these waters surround is practically devoid of life, and this is due primarily to the fact that, over most of it, there is no single month in the whole year in which the mean monthly temperature is above the freezing point of fresh water. This is a salient fact to remember. Low winter temperatures, no matter how severe, are not incompatible with the existence of highly organised life-forms. They are therefore not a serious obstacle to civilised man with his present knowledge. Maximum winter cold is one record Russia can justly claim, though only just, for Canada and the United States are not far behind. Yet in no country is scientific man more firmly installed or more at home than in these three.

The Pre-War Years

The story of the recent history of exploration in Antarctica is a fascinating one. Before 1900 the defences of the continent had been breached by a Norwegian seaman so keen to make a first landing that he pushed aside his superior officer and sprang thigh-deep into icy sea. A year or two later this same Norwegian adventurer, Carstens Borchgrevinck by name—a man of parts if not of punctilio—backed by an English publisher Sir George Newnes, had spent the first winter on the antarctic continent at Cape Adare and made the first tentative push inland on the Ross Barrier, a trivial but a significant record. Much greater things were to follow soon.

The first fourteen years of the twentieth century have been named, not inappropriately, the heroic age of antarctic exploration. The tale is a fitting element in the surge of geographical discovery that was a feature of the peaceful Victorian and Edwardian reigns when major wars were rare and a steady approach towards an eventual Utopia—at any rate for the European ruling classes—was expected by most thoughtful men. This surge of antarctic discovery opened with a sharpening of international interest and in cordial co-operation. European scientific societies, under royal patronage—more common then than now—conferred politely about spheres of interest. Scientific leaders were chosen, or chose themselves; old, tried equipment and methods—the Nansen sledge and cooker; men, dogs and arctic tents—were translated to a new field, and the twentieth-century antarctic race was in full swing.

By 1904 the more accessible parts of the antarctic coastline had been the temporary homes—for one or two years—of European teams with a strong element of natural science. These first scientists were fortunate, as we now know, in the location of their headquarters. Biological and geological discovery went on apace, and we gained our first slight knowledge of the physical condition of antarctic ice and atmosphere. In the meantime sledge parties penetrated for many miles along the coast and made the first tentative journeys inland.

The Swedes, under Otto Nordenskjöld, were fortunate in their location and lucky in their bad luck. They lost ship and gear in the polar pack, but were consequently pioneers in living on the antarctic land. They survived with courage and cheerfulness through two difficult years, during which they collected evidence of several former antarctic floras, afforded a good example of international co-operation by being rescued by an Argentine naval vessel.

The Scots under Bruce left behind them, in Argentine occupation, the first permanent antarctic weather station. This last achievement had results that were to be of some importance when the temper of the world had changed.

French and German, Belgian and Scottish expeditions contributed to this mass assault with notable but unspectacular results. From the international effort there were three important exceptions. Norway and the United States, both of whom

had considerable polar experience, were looking towards the arctic at this time. Imperial Russia—as she then was—who, many years before, in Bellingshausen, had produced one outstanding antarctic explorer, had relapsed into her more usual rather introspective mood.

I have left the British effort until last, for while it was, geographically speaking, the most notable of all, it also leads naturally on to the second phase—the quest for the southern pole. This period was the heyday of the polite recognition of national spheres of interest, and in Antarctica we started with a long lead through the chance that James Ross had approached the continent in the season 1840–41 at the place where the land is most deeply breached by the sea. Scott effected a landing as far south as 78°, and no one by ship could do much better even today. Being of an energetic race—at any rate in all that appertains to physical exercise and sport—his men took to their legs and penetrated deeply into the continent. Employing archaic sledging methods inherited from the Franklin search in the arctic, but gradually evolving an efficient sledging technique within these limitations, *Discovery* sledge parties set a pattern that accomplished a great deal but was eventually to cost us the pole and several valuable lives. They learned that scurvy was not a menace confined to arctic travel and long ocean commissions. On the southern journey the mere suggestion that his breakdown from scurvy was a cause of comparative failure, produced in an extraordinary man an obsession which was to govern the rest of his life and give a name to Shackleton Base today. The curious chance that one of Scott's officers had served with Jackson in Franz Josef Land led to the employment of ponies as draught animals in Antarctica, with further diversion of the British mind from that real polar sledging treasure—the dog.

There followed in the next few years the polar quest. In 1908, Shackleton, to some extent in pursuit of self-respect, made the greatest leap forward towards either pole that any leader ever made. He traversed the Ross Barrier 300 miles from front to rear, and when the mountain ranges curved across his path, struck up the longest valley glacier in the world, and penetrated over the plateau in face of a galling headwind and below zero temperatures to within ninety-seven sea miles of the pole. It is fair to say that when he turned back—a decision surprisingly discreet to one who knew the man—we knew exactly what the southern pole was likely to be like. The only surprise to Scott and Amundsen three years later was the fact that a ski-stick could be pushed its whole length into soft snow because of the absence of strong wind. Shackleton returned—but only just. Forty-eight hours of fine weather on the Beardmore glacier when the party was on starvation rations and looking for a depot was typical of 'Shackleton's luck' as we knew it in that wonderful year.

Of the Amundsen–Scott race I say little because there is no polar story so well known. Within a month two parties of five men stood at the South Pole. One did not return. The British lost the race and gave their lives because their methods were less than the best. We did most of the approach work to both poles and were beaten at the post because we did not fully understand the value of dogs as transport animals nor how to get the best out of those we used. Other factors contributed to the tragedy, but the stark fact is that, with sledging rations completely devoid of vitamin C, the margin of safety of any manhauling party to the South Pole from any base on the rim of the continent is too small. Scott committed errors of organisation which he more than atoned by the manner of his death. He should not have changed his parties from two units of four men to one of three and one of five. By doing so he endangered the party going home, who had, without measuring or weighing apparatus, to take exactly three-quarters of the food from dozens of weekly bags. He handicapped his own party psychologically by crowding them in a tent meant for four men and disorganising a very carefully thought-out routine. He made, in my opinion, a bad mistake in taking with him three officers and one

long-service seaman. Seaman Evans was in a thought-tight compartment by himself and was naturally the first to break. The weather was unkind. Only recently an American scientist has pointed out that, of all the years of which a meteorological record has been kept, 1912 was the stormiest on record and the winter of 1912 set in unusually early. Nevertheless, scurvy was, I believe, the decisive factor. Under man-hauling conditions, four months is about as long as men hauling sledges can live on rations completely devoid of vitamin C. Once that period had elapsed, nervous and physical deterioration were bound to set in, sores to refuse to heal, and lassitude to supervene. Scott's southern party did not starve to death.

This about completes the tale of British antarctic exploration before the 1914–18 war so far as positive achievement is concerned. But there are other things to record before we are finished with the pre-war years. Australia launched her first antarctic venture in 1911, and by 1914 Douglas Mawson had proved his status as polar leader, polar scientist and traveller, and had, incidentally, set a record of endurance that will never be surpassed. Losing one member of a three-man sledge party down a crevasse with most of their provisions, he saw another die of exhaustion at his side, and then, alone, staggered along for three weeks, to arrive at his base one day after his ship had left for home. In the autumn of 1913 I was with Professor David at his Blue Mountain home when the news was flashed across in the first wireless communication ever sent from Antarctica to civilisation without relay, in itself a significant milestone in antarctic history. Mawson, staying south a second year, more than doubled his results, which, with the time-lag characteristic of our antarctic scientific memoirs, are still coming hot from the Australian press today.

These years also saw the birth in the minds of William Bruce and Ernest Shackleton of a significant idea still unfulfilled—the Transantarctic plan, which belongs more properly to the years of war that were to bring the long Victorian–Edwardian age to its end and profoundly change the temper and outlook of mankind.

What were the dividends that accrued from this first twentieth-century wave, characterised as it was by a rather attractive blend of adventure, science, individual rivalry, and international good manners and good will. There were quite a few, but from among them I should like to pin-point two. I was once a geologist, so I may be excused if I go to that field for my first example. The most intriguing feature of polar geological research has, unquestionably, been the evidence of past changes of climate so revealed. East Antarctica we saw, and see today, as a continental shield that had remained relatively stable through the ages with, piled upon it, horizontal sediments, which are a record of its denudation and the profound changes of level that have occurred as the aeons passed. West Antarctica we think of as a prolongation of Andean folds. Between the two lies the so-called Ross-Weddell Graben. We still cannot be certain, as we were certainly not in 1914, whether or not the severance is complete, though evidence from air survey makes it more probable that Antarctica is one continent rather than two.

On this continent, in 1914, we knew of *Archaeocyathus*, a common ancestor of sponges and corals, occurring in Cambrian limestone-stunted forms it is true, but still evidence of warmer seas. Coal seams seven feet thick of Permo-Carboniferous to Rhaetic Age were found in the Beacon sandstone that caps the horst that dominates Victoria Land. From the Priestley Glacier came part of a Rhaetic tree containing in its tissue a tiny winged spore. From the upper ranges of the Beardmore Glacier—collected by a disappointed and weakening party, carefully depôted a few miles from where they died—came 40 lb. of priceless specimens, including *Glossopteris indica*, type fossil of the Permo-Carboniferous so widely spread in Australia and elsewhere. From the fold-ranges of West Antarctica, Nordenskjöld and Gunnar Andersson collected in succession Jurassic, Cretaceous and Tertiary floras culminat-

ing in *Fagus antarctica*, the southern Beech. And, stranger still, in all our wanderings, no single formation of undoubted glacial origin was found older than late Tertiary Age. This remains the classic problem of antarctic geology; fit subject for discussion at British Association symposia—a problem that as a very ex-geologist I will not attempt to solve. Perhaps palaeo-magnetism may yet provide the missing clue and Wegener be finally justified, or finally set aside.

Pre-First-World War exploration paid handsome dividends in a more prosaic field. The Compagnie Argentina de Pesca has just celebrated a jubilee, its fiftieth year of whaling, which makes the first shore-based Antarctic whaling station stem from Nordenskjöld as did the permanent weather station to which I have already referred. To some extent the name misleads, for to the credit of Norway must go most major technical improvements in whaling, from the basic small fast whale-catching vessel to the harpoon gun. Of the vicissitudes of whaling one gets some idea when it is realised that in one's Victorian youth, whalebone for ladies' stays was a chief objective, while today whalebone is the least valuable portion of the whale and it and the tail flukes are the only parts discarded. There are rumours today, however, that whalebone may come back into use as a raw material for artificial silk.

The First World War

In 1914 antarctic exploration was diminished rather than inhibited by the First World War. That war was not quite global in extent, and, in the first years at least, until unrestricted submarine warfare set in, some of the old polite conventions survived from the days when wars were the affair, in the main, of gentlemen and professional soldiers. Shackleton left for the south in pursuit of his Transantarctic quest after having offered ships and men to the Admiralty and having been told to go ahead. As I have said, his first expedition was a terrific success; in its way unique. Yet Shackleton never showed himself a greater leader than when he brought crew and shore party safely through this second, completely unsuccessful expedition without the loss of a single man. Crushed in the ice of the Weddell Sea, the *Endurance* was lost with most of her records and practically all her specimens.

The boat voyage from Elephant Island to South Georgia was an epic seldom surpassed in the history of navigation. Shackleton, the sailor, made little of it. Shackleton, no mountaineer, dramatised the scramble across the South Georgia ranges overmuch. Shackleton, the undefeatable, made four attempts in tiny ships to rescue his marooned companions and at last succeeded. Then Shackleton, the versatile, donned army uniform and became a clothing and equipment expert in Murmansk. I served both with Shackleton and Scott and very briefly met Amundsen in mid-career. I believe a colleague hit the nail when he wrote: 'As a scientific leader give me Scott; for swift and efficient polar travel, Amundsen but when things are hopeless and there seems no way out, get down on your knees and pray for Shackleton.' Shackleton Base for a Trans-Antarctic Expedition is right.

The Inter-War Years

I must be forgiven if I have appeared to stress too much these early years for in these I played a minor part. For the rest I must hasten, picking out only the salient features as I bring the, to me, fascinating story up to date.

As Europe and America recovered their poise, two trends, both important to antarctic exploration, can be recognised. America awoke to Antarctica and brought to her aid all the resources of modern mechanical science. Shackleton, Scott and Mawson had taken the first halting steps in this direction without success. In 1908 I had my first motor-car ride in McMurdo Sound near 78° S. latitude in an open, Arrol-Johnston car with air-cooled engine and with wheels. For ten miles we did thirty miles an hour on windswept sea ice and then met snow and stopped. Scott's

first tractors had little better fate. Mawson's aeroplane crashed in Adelaide in 1911, but journeyed to Antarctica as an alleged air-propelled sledge—and stayed at or near headquarters when there. But in the thirties Byrd used tractors and aeroplanes with success and on the grand scale. He flew to the Pole and back, and in 1937 Ellsworth flew across the continent, landing several times on the way. These were major feats and pointed to a new era to come, in which wireless telegraphy, not an unmixed blessing, but an aid that, once available, could not be ignored, combined with many other mechanical and electrical aids to transform the antarctic scene.

Meanwhile, in West Antarctica, quite different developments of equal interest to the polar specialist were taking place. Even pre-1914-war polar exploration was not really dangerous compared with civilised life. But scurvy was, as I have shown, a major hazard, and winter sledging could be very uncomfortable indeed. I have left winter quarters for cold-weather sledging with a sleeping-bag weighing 12 lb. and returned with the same bag 28 lb. in weight. All the extra weight was frozen breath and sweat that had to be thawed out and warmed before I could sleep. In 1907–12 we were strictly geared to the polar quest. One gallon of oil had to last four men a week, or three men for ten days. Not a drop could be spared from its primary task of preparing hot meals. In 1926 or thereabouts a very remarkable generation of arctic explorers arose in Cambridge. They evolved an efficient technique of living on the land, and, on the sea in kayaks, they out-Eskimoed the Eskimos. Gino Watkins and his colleagues did more than any other factor to rehabilitate British polar exploration in Scandinavian countries where our reputation had rather slumped through Amundsen's easy and clear-cut victory in the polar quest. In 1934 these men transferred their attention to Antarctica as the British Graham Land Expedition under John Rymill. The menace of scurvy had gone (except under catastrophic circumstances) with the availability of vitamin C in tabloid form and the rational use of what meat the antarctic can provide—which, along the coast, is a great deal. Winter travel had been made comparatively comfortable by doubling the oil ration; a simple but fundamental change from which, however, the early manhauling pole-seekers were inhibited by the nature of their task. From Rymill's generation, in direct descent, stem the early leaders of the Falkland Islands Dependencies Survey, that present British nursery of dog-sledging technique, admittedly unequalled, in this particular respect, in the antarctic world, though our cousins the Australians are hard on our heels.

The 1939–45 War

In 1939 the Second World War broke out and for some years the Antarctic was deserted as never before in this century. The 1939–45 war was truly global and total. All civilised conventions went by the board and in the struggle for survival no holds were barred. German raiders used sub-antarctic bases as supply depots and bolt-holes in their raids on Allied shipping and the whaling fleets. In 1938 the Reich had sent a catapult ship carrying two flying-boats to Antarctica to stake a claim and spy out the land. The subsequent raids underlined the rather sinister emergence of politics and national ambition as motives in the antarctic exploration field. It was a variant on an old theme. Iceland and Greenland in the tenth century owed their exploration and settlement not least to the fact that Eric the Red and his turbulent fellow Vikings could not bring themselves to refrain from homicide. The difference after all is one of scale rather than of type. The raider *Kismet* sank nine ships totalling 57,000 tons, the *Pingouin* sank 136,000 tons of Allied shipping and captured a whaling fleet with 22,000 tons of oil.

In 1939–40, before her entry into the war, the United States staged a major expedition which had the distinction of embracing, for the first time, both East and West Antarctica, bases being established both at Little America and at Marguerite Bay in what we call Graham Land and the Americans Palmer Land, a clash

of nomenclature that reflects a controversy a century old. In 1944–5, when the Allies' naval supremacy was re-established and the sea war over, a naval expedition under Commander Marr—who first went south as a sea-scout under Shackleton—was sent to West Antarctica, and the Falkland Islands garrison was strongly reinforced. Landings were made at Deception Island and Port Lockroy and the Melchior Islands. The Falkland Islands Dependencies Survey began to take shape.

A new factor in Antarctica had emerged; Argentina and Chile were looking to the south. Is it unreasonable to infer that, in these fateful years, with a bias towards totalitarianism themselves, they had decided—wrongly—who would win the war, and had determined, not unnaturally, to pick up what they could of the geographical loot that might be lying about when the British Commonwealth and Empire had been destroyed? Argentina, at least, had some claims to succession: though not to usurpation when the elimination of the present occupier did not in fact take place.

The Cold-War Years

When we study the history of antarctic exploration from the end of the Second World War the pattern becomes more complex. There is plainly to be seen the reflection of the great dilemma that confronts civilisation in this era of rapid technological development under the circumstances of cold war. As man's mastery of machines progresses and his movements become more rapid, the earth, considered in terms of time taken to traverse it, shrinks. This fact makes international organisation and international understanding more imperative year by year; an international outlook more essential to the survival of civilised man. At the same time the course of recent events has encouraged an aggressive nationalism which is rapidly bringing about the dissolution of the old colonial empires and the emergence of new States, inexperienced and prickly, eager to assert their new-gained rights and to enlarge their realms.

This dichotomy is mirrored in the antarctic history of the last ten years. The year 1948, for example, saw the conception of the first truly international antarctic expedition. On the initiative of Norway, the Norwegian–Swedish–British Expedition of 1949–52 was launched with purely scientific objectives. Two years of first-class scientific work was done in amity by a truly international team who emerged from the experience with enhanced mutual respect and a record of work of unsurpassed value. The inland-ice of Queen Maud Land was investigated to a depth of 100 metres by platinum resistance thermometers and a core taken down to 200 metres. Six hundred and fifty successful radio-sonde ascents were made up to a maximum of 72,000 ft. A seismic profile of the inland-ice was accomplished during a penetration inland of 390 miles. Aeroplanes of the Norwegian and Swedish air forces combined to extend a survey well begun by the expedition's weasel- and dog-sledging parties. Organisation was efficient and the work done in comfort, as when my friend G. de Q. Robin lived for several months on the plateau in a caravan fitted up as seismic laboratory and living quarters combined, much as if he were on a vacation tour at home. Yet, even today, exploration is not without its risks. Three men lost their lives when their vehicle plunged into the sea over the coastal ice-cliff during a dense fog.

Such amity among the nations, however, remained the exception rather than the rule, for competition is more common than co-operation in the world today. In West Antarctica, Britain was faced with claims both by Argentina and Chile, not only to that sector of the continent, but to the off-lying islands and the Falkland Isles as well. By 1945 the South American neutrals had tabled claims to all land to the south of that continent. These claims were based partly on geographical propinquity, partly as successor states to Portugal and Spain—as inheritors of pretensions that stem from the Treaty of Tordesillas in 1494 and the Papal Bull upon

which that treaty was based—partly, in the case of Argentina, on occupation for a number of years.

Twisting the lion's tail is today a safer occupation than it was a century ago. Yet this country has reacted vigorously enough, through occupation and administration, upon a major scale. The Falkland Islands Dependencies Survey is a name that stands for an effort in antarctic activity that is, in its way, unique. Since 1944 this activity has been maintained on a steadily increasing scale. Today Britain has ten permanent bases in West Antarctica, with a magistrate and supporting team at each. From those on the mainland the old Watkins–Rymill tradition of dog-sledging has been maintained and improved, journeys totalling thousands of miles having been made.

The bases breed their own dogs and carry out continuous trials of equipment and rations. In fact an experimental laboratory in methods of polar travel has been in existence there for a dozen years. A very complete meteorological record is kept at all stations for the benefit of the whaling industry, from which, until recently, the costs of occupation were mainly defrayed. At some stations an ionospheric and seismological programme is in full swing. Topographical and geological surveys are major objectives. This year a start has been made on an aerial survey of the whole region in dispute.

Side by side with the British bases, sometimes within a few hundred yards, stand those of Argentina and Chile, manned chiefly by Service personnel. Our Chancellery proposes—theirs refuse—arbitration through the International Court. Formal protests are bandied to and fro. Through it all, with very occasional exceptions when for the moment national fervour outruns good sense, the personnel of the expeditions interchange social visits and afford each other mutual help. Straying Argentine St. Bernards are returned by British scientists: anniversaries are celebrated in common: British invalids are cured by Argentine doctors and vice versa: Argentine beef and champagne prove acceptable exchanges for Scotch whisky and English beer.

What will come of this international rivalry it is impossible to predict, world balance of power being so delicately poised. In 1948 the United States made a proposal to seven Governments for a limited kind of condominium control. From time to time one nation or another formally reserves its rights. The suggestion of United Nations control finds increasing favour in many quarters and has been mooted in the British Parliament. In the meantime, the situation is bound to have its effect on polar achievement in many ways, of which I may mention two.

It greatly increases the amount of money, manpower and energy expended upon the opening up of the southern continent. If, for example, Russia, however pure her motives, gazes polewards, the United States must perforce take notice too. The strategic value of Antarctica, should world atomic war break out, with the consequent likelihood of the destruction of the Suez and Panama Canals, will stem from the fact that all inter-continental seaborne traffic, and much coastal traffic as well, must then proceed via the Cape of Good Hope and Cape Horn. Under the circumstances, from this cause alone, concern with Antarctica, as a possible air or submarine base, is bound to be a preoccupation of any great power.

Antarctic exploration and exploitation is bound, therefore, to become more and more an affair of Governments rather than, as in the past, of individual adventurers or scientific societies. At the same time, if the experience of the last ten years is any criterion, less and less scientific result will accrue per unit of energy and money expended. Foreign Ministries, even more so Defence Ministries, are very remotely concerned with science for its own sake. This marked failure to work up and publish results is, indeed, the outstanding feature of the antarctic record since the last world war. This is an aspect of modern antarctic exploration to which the British Association, as other science associations, might well give some heed.

I cannot, within the bounds of a single address, hope to deal with the work of all countries in Antarctica since the war. The Americans have planned and carried through operations almost on a war scale. Admiral Byrd goes south periodically with many ships and thousands of officers and men. In 1946, for instance, planes from the deck of an aircraft-carrier, standing off the coast, reconnoitred some 800,000 square miles in a few weeks. Airborne magnetometers were used for the detection of islands completely hidden by land-ice. Icebreakers steamed in a matter of days half-way round Antarctica from the Ross to the Weddell Sea, and, incidentally, pulled out Finn Ronne's party which had done a unique job of dog–air co-operation with the British under Butler along the Graham Land coast. This American party made history by overwintering two women at their base. In theory a further step in co-operation was achieved when an agreement was signed making photographs and specimens mutually available; but, alas, things have not worked out so well in practice as was hoped. Returned antarctic explorers are as human as anyone else. One of the announced motives of the American exercise was the testing of men and equipment in case of the outbreak of 'hot' war.

The contributions of Australia, with the veteran Douglas Mawson as the main driving force, and of France, have been outstanding too. In Kerguelen, France has a sub-antarctic scientific outpost of first-class quality and is preparing airstrips there 3,000 metres and 2,000 metres long. An intriguing side-issue is the attempt to acclimatise 'reindeer, mink, sheep, pigs, and ponies'. From a base in Adélie Land the French have explored widely along the coast and some distance inland. Australia has permanent stations established at Macquarie Island and at Mawson on the mainland. A spokesman has recently defined her objective as the making of short forays at selected points on the antarctic continent, concentrating on specific scientific problems, which suggests that here, at any rate, the old scientific spirit is still alive.

Whaling

No account of twentieth-century man's relationship to Antarctica would be complete without some reference to the history of the whaling industry. Whatever may happen in the future, so far whales, elephant seals and fur seals are the only tangible assets antarctic exploration has produced.

The first shore-based venture in South Georgia in 1904 employed one catcher and took 195 whales. In 1954–5 nineteen factory ships, three shore stations and 254 whale catchers between them accounted for 37,654 whales and produced 372,956 tons of oil and nearly 80,000 tons of meat products. Until the present spate of government activity set in, antarctic exploration had, through this single asset, paid for itself over and over again.

The whaling industry has not been without its vicissitudes. The price of oil has fluctuated from £8 10*s*. 0*d*. a ton to £170 a ton, though it is now fairly stable at £75. But operating costs increase steadily and today the margin of profit has become very small. The success of whaling, of course, even if prices are remunerative, must depend upon the maintenance of the stock of whales, and this has led to international regulations on a considerable scale. In the early days the industry was carried on from shore stations, all of which were, in those less complicated times, in the undisputed control of Britain. The industry was easily controlled by the issue of licences. In 1925 the situation was transformed by the invention of the floating factory, and the stern slipway followed soon. Up the latter the whales could be hauled by winches and flensed and rendered down on board, thus doing away with the need for a shore base away from the home country. From now on an entirely different type of control was needed. The League of Nations in 1931 drew up a convention which established the principle of the international regulation of whaling on the high seas. In 1933 most companies agreed to voluntary limitation of catches. Whaling inspectors were appointed to accompany all ships in 1934–5.

Complication was caused by the defection of Germany and Japan from the League just before the Second World War, but when the war was over, the International Commission was established in 1946. The catch is now limited to 15,000 Blue Whale Units. Despite protective measures, the Blue Whale stock has greatly decreased, and there are today signs that the Fin Whale stock, now the mainstay of the industry, is on the decline.

The skill of the whalemen of Norway has been both the strength of, and a danger to, the whaling industry. Something in the nature of a Guild of Whalemen has developed with its accompaniment of a jealously guarded monopoly of skill. The two chief trends of the half-century have been the improvement of the technique of whaling and the more efficient utilisation of the body of the whale. In the former field Norwegians have been supreme, though recently useful innovations such as the electric harpoon—to replace the destructive harpoon-gun—and the echo whalefinder, have originated in this country. Other notable developments have been the use of helicopters for finding whales and the attachment to whale carcases of small wireless transmitters so that catchers fitted with direction-finding apparatus can home on them in any weather conditions. In utilisation Britain has, perhaps, taken the lead. Today almost every part of the whale is used. The main use of whale oil is, of course, the manufacture of margarine and soap, but it is also used as burning oil, for tempering steel, for leather dressing, and in lubricants. Experiments in the production of artificial wool from blubber are under way in Japan. Whale-meat meal is used for feeding cattle, pigs, poultry and silver foxes; small quantities of whalebone for the grooving of sliding doors, for collar studs and stiffeners, for cigarette cases and the backs of hairbrushes. Canned whale-meat is today, even in peace time, in some countries a common form of human food. In 1954, 30,000 tons of salted whale-meat was used as food for miners in Japan alone. In 1954–5 these by-products were worth nearly a million pounds.

Whaling springs from exploration but it has, in its turn, had a profound influence upon exploration. Here again, there has been a curious mixture of effects. Whaling captains, by and large, are, not unnaturally, loth to give away the location of good hunting-grounds. From this natural reticence have sprung many of the contradictions and uncertainties of polar history in the past. This trait must have had its effect upon the controversies about priority of claim that have been so frequent and so bitter. Yet there have been from time to time enlightened owners and skippers who have contributed notably to the unveiling of the southern continent and the islands off its coast. From the nineteenth century Messrs. Enderby Bros., John Biscoe, and Weddell immediately spring to mind. Their names are rightly written large upon antarctic maps. In the present century the firm of Salvesen, Captain Larsen—who died while whaling in the Ross Sea—Lars Christiansen, and Mikhelsen, equally deserve to be remembered as explorers as well as whalers.

Another valuable contribution to the exploration of antarctic seas that springs directly from whaling is the oceanographical research carried out by the Royal Research Ships *Discovery I* and *II*, and now continued by the National Institute of Oceanography. This work was originally financed by the United Kingdom from whaling dues. The *Discovery*'s work is essentially a survey of the antarctic seas and their organic resources. These seas have an exceptional density of marine life, and oceanographical conditions are distributed in a simpler pattern than in most other oceans. For twelve years between the wars R.R.S. *Discovery* covered the southern charts with a complex network of courses, encircling the globe. The scientific results surpass even those of the *Challenger*. By 1938, eighty memoirs by fifty authors filled fourteen volumes. More than 4,000 whales had been marked. Her researches on the breeding and life cycle, the distribution, migrations, and habits of the whale have been of major importance to the successive bodies responsible for the regulation of the industry. The oceanographic survey has revealed the

broad structure and mechanics of the principal water masses and currents and certain basic features of the distribution of the ocean plankton.

The Antarctic Today

So we come to the present day and to a fresh resurgence of interest which we can really claim has science for its main drive. Twice in the past, in 1882–3 and 1932–3, the scientists of the civilised world have planned an International Polar Year. Today they plan again, but, as is fitting, on a vastly larger scale the International Geophysical Year for 1957–8. Although it is intended to cover the whole earth with chains of stations from pole to pole, the systematic exploration of the physical phenomena in and over Antarctica is a most striking component of the plan. A long range practical objective has been described as a better understanding of climatic variation and the study of the nature of long-period climatic change. We are apt to forget how much the present pattern of our civilisation depends upon the latent heat of the ice. If the present ice-sheets of the world melted in a hurry, sea-level would rise somewhere in the region of a hundred feet. This would, among other painful things, bring about the greatest slum clearance the world has ever known. The present long-term tendency to glacier retreat is, however, accompanied by a rise in sea-level of only four inches a century. The rise in the temperature of ocean water that accompanies this climatic change has important repercussions on the fishing industry upon which the welfare and livelihood of tens of thousands of European families depend. Another question still a matter of controversy is whether continents move, and, if so, at what rate. A fuller knowledge of the ionosphere will help in improving wireless communications which are now essential to our mode of life. Upper atmospheric studies are important in the future development of high-flying aircraft.

The immediate observations that will be made in 1957–8, chosen because it is a year of nearly sunspot maximum, are, however, concerned with the exact recording of physical phenomena which will appear to the layman as the purest of pure science. Yet Sir Edward Appleton has lately reminded us that history has taught us to regard pure science as the capital of scientific knowledge, capital that has yielded astonishing dividends from time to time.

The world is entering on its latest experiment in a big way. A mere enumeration of the subjects of study is evidence enough of the comprehensive nature of the programme. Meteorology, the study of aurora and airglow, geomagnetism, ionospherics, ozone, cosmic rays, glaciology, solar activity, variation in longitude and latitude, seismology and oceanography, are all embraced. I can even visualise a little geology being done on the side, though most of the people concerned may never handle antarctic rock. The total cost of the plan has been assessed at £100,000,000. No less than forty-five nations have pledged themselves to play their part. In and around Antarctica there will be over forty stations, of which three will be, respectively, at the geographic pole, the geomagnetic pole, and the Pole of Inaccessibility.[1] These last will be supplied and maintained entirely by air.

The most spectacular element in the plan is the examination of the upper regions of the atmosphere. Man reaches further towards space than he has ever done before. It is planned to employ man-made satellites, despatched by means of three-stage rockets, to a height of 200 miles, there to girdle the earth in an elliptical path which varies between 200 and 800 miles, in 90 minutes dead. The satellite is expected to stay up days or weeks, before it falls and is consumed, as are millions of the meteorites that enter the atmosphere every day. Large rockets will reach out

[1] The term was first used by Vilhjalmur Stefansson to indicate the point in the Arctic Ocean which was theoretically most difficult of access under the sledging conditions prevailing in his time. As applied to the antarctic continent, it must presumably mean the spot on the inland ice most remote from an attainable base.

—their tracks like slender fingers—to several hundred miles above the earth carrying heavy loads of recording apparatus. Rockoons, smaller rockets launched from balloons twenty-seven miles up, can carry a lesser load to a height of over sixty miles. Small rockets may be despatched from aircraft in flight.

Balloons will be launched trailing a variety of instruments, tracked throughout their course by observers, eventually parachuting to earth. Automation, a cause of anxiety and unrest here at home, can be seen with more detachment as an essential element of the I.G.Y. plan. Perhaps it is at its most dramatic in the so-called 'Grasshopper', a small recording device weighing 200 lb. which can be dropped from an aeroplane at any desired spot. As described to me, it automatically opens, rights itself on its six legs in true insect fashion and goes into action. 'It gathers weather information at predetermined intervals, transcribes it automatically into International Morse Code, and transmits it by radio at seventeen words a minute'. It works for sixty days and records and transmits surface wind speed and direction, temperature, barometric pressure and humidity. To one who has toiled to and fro between hut and screen, this sounds to be a very desirable development indeed—a distinct advance towards *Utopia Antarctica* and not a matter for grousing at all, even if the substitute job is handyman or cook.

Already I.G.Y. has begun to work its transformation of the antarctic scene. Direct American flights from New Zealand to Antarctica have taken place. The Russians are in regular wireless touch with Moscow and plan a supply route direct from the U.S.S.R. by air. The continent is becoming studded with little towns. Icebreakers plough the antarctic seas with internal temperature kept at 75° F.: a fact that reminds *me* vividly of what I reckoned to be one of the hardships of American life. In this great effort Britain plans to play a worthy part. One major expedition, sponsored by the Royal Society, is already established near 76° S. in the Weddell Sea. The Falkland Island Dependencies Survey bases are being reinforced to help.

Dr. Fuchs' transantarctic expedition is a horse of a rather different colour, though it will, as a side-issue, contribute to the plan. He intends to cross the antarctic continent from west to east. It has been said in criticism that the British have been so obsessed with the story of Shackleton and Scott that they cling unduly to outmoded ways. We have, it is true, suffered from too strict adherence to tradition in the past. There is an element of gambling in the present transantarctic plan. But Fuchs intends to take seismic soundings of the ice depth together with a continuous gravity traverse as he makes his way across. In addition to tractors and aircraft, he will have auxiliary dog transport to examine rock exposures he may meet along the unknown country he has to traverse. Of all the modern equipment the small electronic device weighing less than one pound by which he can home on his moving column from distances up to twenty-five miles attracts me most. If he is fortunate he may come back with results that will, of their kind, be as valuable as anything his more sedentary competitors can possibly produce. And there is an element of adventure that, for any normal citizen, will definitely add to the fascination this particular venture exerts. Good luck to him. May he and his men so conduct themselves that, if they do fall short of complete success, they will, like Shackleton of *Endurance* fame, come out of the trial with reputation not diminished but enhanced.

The Future

When I left England with Shackleton in 1907, I travelled to Australia in six weeks by sea at a cost of £19 from a London where the four-wheeler horse-cab was still commonplace; where wireless was in its infancy and a criminal could still hope to outpace the law by ocean travel. I lived in a world which was, by and large, convinced that mankind was unique and life confined to Earth. The expedition left

New Zealand on New Year's Day, 1908, and for all its countrymen knew about what was happening in the next twelve months we might have taken off into space. The first tentative flights were being made in machines heavier than air.

Today I return to England for the British Association meeting from Australia in the inside of three days at a cost of £300; to find Dr. Fuchs in constant touch with his colleagues in Antarctica; in a world in which a Harvard professor of astronomy has recently asserted in a London Sunday newspaper that life exists on a million worlds. His country proposes to launch an earth satellite as part of its I.G.Y. programme. American airmen are flying hundreds of tons of stores from New Zealand to Antarctica as an ordinary Service transport operation.

Who can predict what may happen in a similar tale of years in the future? Antarctica may be colonised: man may have landed on the moon. On the other hand, it is as likely that, if man's passions get out of hand—and control of emotions is not a notable twentieth-century trait—the survivors of mankind may be plunged in a Dark Age such as has succeeded civilisations of the past at least nine times in the chequered history of mankind. Man is indeed at a crossroads today. We of the British Association may well ask ourselves what we can do to help the world to make the choice that will ensure the survival and further progress of humanity perhaps to the point when man is no longer earth-bound.

So far as the future of Antarctica is concerned, we still have too little to go upon for speculation to cut much ice. Now that atomic energy is available and atomic power is immediately ahead, the mere strategic threat of a monopoly of Antarctica by any one power should by itself suffice to keep the interest of the nations alive. Great and valuable mineral deposits there must be in any continent of this size and kind, though nothing of significance has so far been found. Prospecting on the continental scale, let alone exploitation, will be so costly that, apart from a stroke of luck, it will require finance and organisation at least on the national scale to ensure success. In 1951 the two hundred million pound Kitimat project was started in a completely undeveloped area in British Columbia and is just about to pay off. But this is based on cheap local power and will be used to process aluminium ore brought 4,500 miles from Jamaica. In Antarctica something on this scale would be the least that would be required should large and valuable ore bodies be found. Airborne magnetometers and stereoscopic-camera mapping are already in use in antarctic surveying. Future exploitation lies with the co-ordinated use of airborne surveyors carrying out wide sweeps and of helicopter-borne ground parties equipped for high-speed core drilling and rock sampling. In Canada five geologists with two helicopters have prospected in one season some 67,000 square miles. Attention must needs be concentrated, as at present, on the relatively few exposed rock surfaces where mining could be carried out without difficulties arising from ice movements. Once the ore is located the whole enterprise could gradually go underground. Tunnel or adit entry might be followed by the construction of underground engine-rooms, hoisting gear and mineral dressing plants. Floating power-stations that could be withdrawn for maintenance during the 'closed' season might be a useful expedient in the initial stages, but land-based stations would be essential to any permanent settlement. Once a footing had been obtained anything might happen in these days of technological man.

It has been suggested, for example, that the antarctic might have a future use as a vermin-free store for the world's periodical food surpluses where these might be preserved against the needs of future generations, since widespread crop failures might otherwise spell disaster to a world whose population seems destined to increase. Again, one of the main claims for atomic power is that it can be maintained for long periods in remote places with a minimum of attention and thus might be used for the development of the desert areas of the world, uninhabited today because they are too cold, too hot, or too dry. If atomic-powered icebreakers

can be built, why not atomic-powered settlements on the antarctic mainland? The harnessing of the antarctic gales is another possible source of power. But everything will depend upon the discovery of a worth-while economic objective, and this is not at the moment in sight. The chances are that, for the remainder of this century, Antarctica will remain the scene of investigations in pure rather than applied science. But man may yet find a way to overcome the latent heat of ice and add a seventh habitable continent to the six he already has in thrall.

PHYSICS AND TECHNOLOGY

ADDRESS BY SIR GEORGE THOMSON, F.R.S.

PRESIDENT OF SECTION A

THE influence of a science such as physics on men's lives comes in two quite different ways: first, directly by the influence of the ideas, and secondly, indirectly through their material consequences.

Though I want today to speak of the latter, I am not suggesting for one moment that the applications of science are more important than science itself. On the contrary, I believe that science as a system of knowledge is more important than the applications to which it leads, and that it has a value, and that the effects of science on men's minds are more important than its effects on their material life. All the same, applications of science have a great importance. Not merely in the obvious way, but also as being a very clear and concrete proof of the truth of the ideas. Some of the theories in modern physics are so abstract that one is reminded of mediaeval philosophy, and may sometimes wonder if they are no more than an ingenious web spun by the spiders of the brain; but when such theories lead to conclusions that are incorporated in machines which are impressive either by their size or by the way in which they penetrate into every house, one is forced to believe that these remote ideas have a true contact with reality, and in some sense represent it. Furthermore, the prestige which the more startling applications have given physics in recent years affects, and justly I think, popular regard for the ideas, and make the generality of educated men hesitate, at least, to hold views strongly contrary to them.

In early days technology owed little to science. The earliest discoveries—for example, the smelting of metals—were made before science existed. Later discoveries were made, for the most part, by humble men, who lacked the education of the age. Practical arts were intellectually despised—even the painter and the sculptor did not rank very high in social esteem—and discoveries were made more or less by accident, and often kept as trade secrets. To some extent the art of navigation was an exception, which early depended on primitive astronomy, and came in the Middle Ages, and perhaps earlier, to depend on the astronomical science of the day. Compare, for example, Chaucer's treatise on the astrolabe. Navigation, too, had its influence on science. It is, perhaps, not an accident that the first real scientific treatise published in England, and almost the first in the world, was Gilbert's *De Magnete* of the year 1600. Gilbert, the physician of Queen Elizabeth, studied magnetism primarily in relation to navigation. It was terrestrial magnetism, as we should now describe it, in which he was primarily interested, though his study was a model of the scientific method.

But technology is only one source, and perhaps not the most important, of physics, which has also come from the instinct of curiosity that is innate in mankind, the study of odd phenomena; the reflected face seen in a pool; the apparent breaking of a stick as it passes through water; the attraction of rubbed amber for pieces of chaff; the twitching of frogs' legs in an Italian butcher's shop—it is from such sources as these that physics owes the greater part of its origin. Nevertheless, technology played a part—the study of heat received a great impetus from the invention of the primitive steam engine, so that by a queer paradox the second law of thermodynamics was discovered before the first; and here we see an early example of the interplay between technology and science, for it is on record that Watts was asked to repair a model of the Newcomen engine in the possession of the Department of Natural Philosophy at Glasgow University when he was an instrument-maker there, and this no doubt helped to direct his ideas towards steam.

But the real influence of physics on technology starts with electricity. From its start electrical engineering has been directly dependent upon scientific discovery. The very first electrical machine, the lightning conductor, derived directly from Benjamin Franklin's experiments, and so it has been throughout. The dynamo came from Faraday, though it took nearly fifty years after his discovery before it reached real commercial importance. Radio comes from Maxwell and Hertz. The cathode-ray oscillograph, that universal tool in modern laboratories and engineering works, is a development of J. J. Thomson's apparatus for measuring e/m, and has itself developed into the television tube of the home. Still more true is this of the new industries based on physics, where the physicist is still in charge.

But if technology owes physics a debt, it has well repaid it, for to the progress of technology, physicists owe the machines, without which progress in physics would be impossible. First and foremost, of course, are the precision machine-tools which are so necessary in every laboratory workshop, but hardly less in importance come the devices which industry has taken in the first instance from physics, greatly improved and handed back to us. For example, the vacuum pump. What a difference there is between the old days, when the unfortunate physicist had to exhaust his apparatus by means of a mercury reservoir, laboriously raised and lowered several times a minute, and present pumps, enormous in power, which make it possible to exhaust to the lowest vacuum, large and complicated pieces of apparatus. This advance could hardly have taken place unless vacuum pumps had become an important piece of commercial equipment for making lamps and valves. Other examples are the valves themselves, which are made cheaply and in great variety because of the commercial demand, and the innumerable cheap radio components which enable the research student in physics to spread his ingenuity in devising complex 'electronics'. The debt is being abundantly repaid.

I should like to turn aside for a moment to consider the position of the physicist in industry. Only shortly before the war there were comparatively few people engaged in industry who called themselves physicists. Now the number is increasing exponentially. Let us consider for a moment what the difference is between the physicist and the engineer. Superficially, one might say that physics is the theory of engineering, and that engineering is applied physics—and this, I think, would have been a satisfactory answer fifty years ago, but now there are people in industry who call themselves physicists and feel that they are different from their engineering colleagues, although they may be engaged on the same work—and, anyhow, for the same purpose. I think the difference lies in the character of the knowledge which they use, and the problems which they tackle. The engineer is a man who, besides knowing the theory underlying the problems he has to tackle as far as it has yet been discovered, has, in addition, a background of experience, acquired sometimes by himself, but more often by other people, which helps him in design. Fundamentally the engineer ranks with the artist, his job is to make a *design*, that is an organic whole, and his theory helps him rather as perspective helped the artists of a now bygone age. Beyond this he depends on knowing similar designs in the past. He does not start completely from first principles; to do so would be wasteful. He rather modifies last year's designs to suit changed requirements. The physicist, on the other hand, has little of this know-how. He is best at those industrial problems where development is very rapid, and where in consequence little know-how has accumulated. His value to the industrialist lies in that he is prepared to look at things from first principles, and to try a variety of quite different modes of attack. His business is to get the thing to work somehow, but the product he makes is apt to be scrappy, lacking in unity, very likely to go wrong. It is for the engineer to tidy it up and make something that can be worked by a housewife.

Let me now consider a few of the particular instances in which physics is being applied. Remember that these are only instances from an enormous field. I will begin

with the one that is, perhaps, most in men's minds—the applications of nuclear energy. I shall not discuss their applications to the technology of war; it is a fascinating subject, but a horrible one, and one may hope, without foolish optimism, that these applications will remain purely experimental. But on the peaceful side they are going to be real enough. First in importance comes the application to the production of energy. It is hoped that before this year is out electricity will be fed into the grid from the first of the nuclear generators at Calder Hall. For a year a much smaller generator has been working in Russia, and American submarines have been powered by nuclear energy. Nominally, perhaps, this last is an application to war, but the engines which drive the *Nautilus* could equally well drive any other kind of ship, if it were worth while commercially.

You will remember, of course, that nuclear energy, at least up to the present, is applied in the form of heat; the chain reaction by which atoms of uranium or plutonium split under the action of neutrons, and themselves yield more neutrons, generates enormous amounts of heat. It is in this form that the energy appears. Nobody has yet devised a means of generating electricity from fission directly, though of course it is the electrical force of the repulsion of the two halves of the dividing nucleus which provides the energy. It seems a very difficult thing to do, though perhaps not absolutely impossible. Some electrical energy can be produced directly from the fission products, but these only account for a small part of the total energy involved. The reactor then, as the device is called which utilises the chain reaction, is essentially a furnace, and the heat from it must be turned into electricity by some kind of conventional method. In the Calder Hall piles the heat will be removed by carbon dioxide and transferred through heat-exchangers to water and steam. The steam then goes into the turbines driving the dynamos, which generate the electricity. I need not, perhaps, go into the details of the very extensive programme for the development of nuclear energy here, which has been set out in the White Paper, but I might just recall that by the year 1975 it is expected that 40 per cent. of the, by then, considerably increased generation of electricity in this country will come from nuclear power, and that this will save approximately 40 million tons of coal per annum. There have been few cases in economic history in which a new discovery has come so completely in the nick of time. It seems impossible for us to produce in this country more than about 200 million tons of coal a year. The difficulty is perhaps more a matter of psychology and social conditions than of engineering, but that makes it no easier, and perhaps harder, to overcome. Anyhow, from being a coal-exporting nation we are rapidly becoming a coal-importing one. The dollars that can be spent on this are limited, and it looked as though the country would be strangled by its inability to expand its use of power, the thing above all others which matters in modern industrial economy.

It is sometimes asked what will happen when the uranium runs out. I do not think we need be afraid. It was stated last year at the Geneva Conference that one million tons of uranium had been already proved; each ton is theoretically the equivalent of three million tons of coal in producing heat (although probably not more than a third of this could be practically extracted). One million million tons of coal equivalent will last the world for many years, nor is there the least reason to suppose that there is not much more uranium available. At present figures the cost of the uranium is a small part of the cost of the new power, which comes mainly from the cost of the installation and maintenance. Then there is thorium, which, though not immediately usable, can be turned into a very useful fuel by packing it round a reactor that is working. Even beyond these possibilities comes that of thermonuclear action. As you know, work is being done here and in the United States and in Russia to make commercial use of the energy that is released in the nuclear reaction between the nuclei of deuterium (heavy hydrogen), which is the basis of the hydrogen bombs. The difficulties are considerable, but the energy, as is

only too well known, can be released explosively. I have no doubt that in the not very distant future it will be tamed, and used for peaceful purposes. Whether it will greatly improve on uranium and thorium remains to be seen. It will depend on how complex the devices have to be to release it. In the long run I think it may win, as I believe that there is much more possibility of deriving electrical energy directly from a nuclear reaction in the case of deuterium than in the case of uranium, and this would save the inefficiency of the heat engine that now interposes itself. It is also possible that a combination of the two might prove economic. The deuterium reaction produces neutrons—these might be used to turn uranium into plutonium, or thorium into uranium 233, thus producing very valuable material which can be used to enrich normal uranium.

The reactors proposed in the first stage of the British proposals will work with normal uranium, or with uranium only slightly enriched with special fissile material such as plutonium or separated uranium 235, but as the art progresses and supplies of these substances become more readily obtainable, there is little doubt that we shall tend towards the use of highly enriched material. Already the experimental plant at Dounreay in the North of Scotland is designed to do so.

I might perhaps refer to some of the limitations of nuclear power. The chief one is that it necessarily produces at the same time a large amount of radiation, which is harmful to health, hence this has to be shielded, and the shielding is necessarily cumbrous. There seems no way out of this—I do not see the slightest hope for the nuclear motor-car. I have grave doubts about the nuclear aeroplane; its sole advantage is that it would save the weight of fuel. For a very long trip it might pay, but such long trips are only needed in war. For crossing the Atlantic, I do not believe that the saving in the weight of fuel would compensate for the weight of the shield, and the increased complexity of the machinery, at least in an aeroplane of anything like the present size.

A minor application of nuclear energy, but minor only in comparison with what we have just been speaking of, is that of radio-active isotopes. Radio-active isotopes are atoms belonging to some normal chemical element, from which they are indistinguishable chemically, but which have in addition radio-active properties. A few of them are used for chemical purposes as substitutes for radium, but much the most important use is as tracers, that is as labelled atoms. If some are mixed with normal atoms of the same species, then the path of these normal atoms can be followed from observing the radio-active isotopes, for since they are chemically similar they will follow a similar path. The uses of these isotopes are innumerable in medicine, biology, and in engineering. It will be enough to give just two examples, one from medicine and one from engineering. It may be important in diagnosis to determine the rate of flow of blood: for example, where it is suspected that there is restriction, as in thrombosis, Sodium 24 is used for this purpose. If a constriction is suspected in the leg, a Geiger counter is placed on the patient's hip, and an active sodium chloride solution injected into a vein in his foot; the interval of time that elapses between the moment at which the injection is given and that at which the counter begins to respond shows how long it has taken the blood to flow up the leg, and this can be compared with the time for the other, normal leg.

In engineering, if you wish to measure the wear of a bearing rapidly and accurately, you can incorporate an isotope into the metal of the bearing, and measure the radio-activity which is carried away in detritus by the oil.

The next scientific application of which I want to speak is as much an application of mathematics as of physics, but both, after all, are in Section A. I mean the electronic computer. An electronic computer cannot be dropped as a bomb, and so has not quite made the headlines in the way nuclear energy has done, but I believe it is comparable in importance. The ability to apply precise reasoning to very large amounts of data in a reasonable time is something new, and the powers that it gives

are still not appreciated properly. The introduction of electronic computers into science may prove not much less important than the introduction of mathematics in the seventeenth century. This seems odd when one recalls that all an electronic computer does is to add or substract—even its multiplication is done, in principle, by successive additions. Its importance comes in two ways. Firstly, it is possible to alter the instructions, which are coded in numbers like the numerical data, by what comes out as the result of a calculation. You can, for example, make the machine do different things according to whether the answer to a particular sum is even or odd, by adding the last digit of the answer to a number which represents the instruction telling the machine what next to do. It has, to this extent, the power of discrimination, of being influenced, that is, by what it has itself found; this gives it great flexibility. The other reason for its great importance is simply its speed and the consequent enormous number of operations which it can perform in a given time. Its versatility is enormous. Apart from the obvious applications to mathematics, to physics, and to chemistry, it is needed for work in genetics, in aeronautics, in textiles, in social medicine, in agriculture, even in anatomy, though I am not sure what it is proposed to do with it there! It has recently been applied to meteorology to calculate the weather, by an application straightforward in principle, but difficult to apply, of the laws of dynamics, to the movements of masses of gas. This is at present being done both in Britain and in America. A more mundane application is to commerce, to the calculation, for example, of pay due to employees, and other administrative purposes where a great many sums have to be done within a limited time. It may do much to take drudgery out of the office, and be to the clerk what the bulldozer is to the navvy.

Then there is the new discovery of the transistor. This is a device replacing the thermionic valve by a minute slice of duranium or silicon. It was originally discovered in America by Shockley and Bardeen, and its possibilities quickly appreciated. It depends on the small degree of conductivity of germanium due to the presence of a few free electrons, or what are called holes—places from which an electron that ought to be there is absent. The movement of these constitutes a current which can be controlled by electric fields, and the transistor can do the work of the normal free-electron valve. Its advantage is that it is enormously smaller and requires no hot filament. It will make possible a great reduction in size, and probably in cost, of all electronic gear. Though Bardeen and Shockley were employees of Bell Telephone Laboratories, America, at the time they made the discovery, they were working on the problem purely for its scientific interest, and the application was unforeseen till the discovery was made.

Finally I should like to say something about the proposed satellite which the United States are hoping to send up next year. It is an admirable example of the interplay of physics and technology. On the one hand, its immediate purpose is to make measurements outside the earth's atmosphere. On the other hand, it is the first stage in what one hopes will be the overcoming of gravity, in the sense of making it possible for men to leave the Earth and visit other planets. Further, the rockets which drive it depend on principles which were developed mostly for war purposes. It will be launched by a three-stage rocket, and it is hoped that when the last stage has finished firing, it will be moving at a height of about 300 miles round the earth, with a speed of nearly 18,000 miles per hour, which would ideally enable it to move in a circular orbit round the Earth. Small errors in the final velocity, either in magnitude or direction, will result in an elliptic orbit which will, if more than of a very small eccentricity, result in its approaching the earth at perigee a good deal nearer than 300 miles. This will result in its being rather seriously slowed down by the just appreciable amounts of air that it will find, and so will reduce its lifetime. But ideally it ought to be able to stay up for a matter of weeks, very slightly spiralling in towards the earth as its speed is slowed down by the very

minute amounts of air that it will find. All the time, this sphere of 25 cm. diameter will be sending messages to the earth of the measurements that it is taking of the radiation of all kinds that it receives. It may even be able to verify experimentally some of the laws of relativistic gravitation, the changing rate of a clock in particular.

Thus physics and mathematics are becoming more and more important in technology. Branches of technology are arising which depend on new scientific discoveries, and inside existing technologies advances are being made in detail by using devices based on physics and methods which depend on mathematical, especially statistical, analysis.

It is of the greatest importance that more young people should be trained in these sciences, both for their own future advancement and for the good of the country.

STEELMAKING SINCE BESSEMER[1]

ADDRESS BY SIR CHARLES GOODEVE, O.B.E., F.R.S.

PRESIDENT OF SECTION B

THE British Association has a long history of contributions to the 'advancement of science'. It is justifiably most proud of the fact that its summer meeting exactly one hundred years ago was chosen as the place to announce a discovery which was to have an effect upon civilisation ranking amongst the most important in the history of mankind. At Cheltenham on August 11, 1856, Mr. Henry Bessemer told of a new method of making steel, a method which was, at least in principle, extremely simple and cheap. This exciting announcement was fully reported in *The Times* a few days later.

The speed with which Bessemer, working at St Pancras almost alone, brought about the original successes which led to his announcement; the awful set-back and disappointment which followed when he tried to make the process work in the steelworks of his licensees; the even harder work, now with the help of some others, which finally won through to ultimate success—all add up to a classic which is a prototype for the story of many inventions. Bessemer had already achieved financial independence through his invention of a method for making bronze powder for gold paint, and one which was very much cheaper than the existing methods. He took advantage of this independence to press forward with his experiments at a fast rate. Bessemer was a determined and courageous man, and the set-back only spurred him on through the usual frustrating period of bringing a new discovery into commercial use. His story has set an example and given courage to many inventors.

Much has been said in this centenary year[2] about the discovery itself, the developments which followed and the revolutionary consequences of the introduction of tonnage steel. I propose to talk about the chemistry of this high-temperature process, and in particular about that chain of chemical development which was started by Bessemer and indeed has not yet finished. I refer to the making of steel by pneumatic processes.

Steelmaking, because of its importance, has led the way through most of the last hundred years in technical progress in high-temperature chemistry. In recent years, however, processes to make metals such as titanium, which require still higher temperatures, are sharing this leading position.

Let me recall for you the chemical problem faced by steelmakers today, a problem which is much the same as that faced by Bessemer. Iron ore is essentially burnt iron or iron oxide, mixed with oxides of other metals, principally, silicon, aluminium, and calcium. The first stage is to 'un-burn' the iron or deoxidise it to form the metal, which, being heavier and insoluble in the remaining oxides, will, if hot enough to be molten, separate from the other components. However, in doing this one has to deoxidise selectively to make sure that one does not bring into the metal a large number of impurities. Fortunately, this can readily be done, so that little of the silicon, and none of the calcium and aluminium, comes into the metallic state. However, one cannot prevent all the phosphorus and some sulphur, commonly present in the ore and in the fuel respectively, coming into the metal.

A cheap and simple deoxidising agent is carbon in the form of coke, and this when mixed with iron ore in the blast furnace carries out the necessary reduction very efficiently and rapidly. It is not practicable to stop the process of deoxidation

[1] Address delivered at the Sheffield meeting of the British Association on August 30, 1956.
[2] See Bibliography.

at the right place, and the metal which comes out of the blast furnace is over-deoxidised and contains a large quantity of carbon. This amounts to only about 4 per cent by weight, but represents nearly 16 per cent of the atoms in the metal. This iron–carbon alloy corresponds to a stable chemical stage in the process and, therefore, one which is exceedingly difficult to avoid, if carbon is used. It does, however, present other problems and these are the problems facing the steelmaker himself.

Once liquid pig iron is made, the problem is to reoxidise it to remove the carbon and possibly other impurities. Before the days of Henry Bessemer this was done by rabbling or stirring with molten iron oxides in what is called a puddling furnace. This process, which is still carried on today in some remote parts of England (!), operates in three main stages. The first of these is one of the oldest purification processes known to chemists, that of crystallisation. To understand this, I must remind you that pure iron crystallises at a temperature of 1535°C. The melting point of pig-iron, on the other hand, is reduced by the presence of the carbon and other impurities to somewhere near 1100°C. The puddling furnace operates at a temperature just above this. When half the carbon has been removed by oxidation through reaction with the iron oxides, iron crystals begin to form, and these are, like most crystals, purer than the melt from which they have come.

However, they still contain more than 1 per cent. carbon and this is removed in a second stage. The crystals are stirred with the molten oxide slag so that the carbon, on diffusing to the surface of the crystals, is oxidised and boils off. Very low carbon contents can be obtained in this way.

The final stage consists of raising the temperature and forming a ball of iron crystals, with some entrapped slag, on the end of a rabbling rod. These crystals cannot be separated by gravity as a liquid iron does in the blast furnace but a partial separation from the slag can be achieved by skilful squeezing under a hammer.

The process requires great skill and is an extremely slow and arduous one. Two to three hours are required to produce one-quarter ton of metal. The first stage is limited by the difficulty in stirring rapidly enough to promote a chemical reaction between a viscous slag and a heavier molten metal beneath it, and the second stage depends on diffusion in the solid metal phase. There are important physical limitations as well, especially in the third stage of slag removal under a hammer. The iron itself, known to you all as 'wrought' iron, has some good qualities, but is much inferior to modern steel.

The open-hearth furnace, which was invented shortly after Bessemer's converter, operates in a way similar to the first stage of the puddling furnace in that a slag containing iron oxide must react with a bath of melted steel scrap to which has been added liquid or cold pig iron. However, the chemical reaction in the open-hearth furnace is much faster than that in the puddling furnace because it operates at a temperature nearly four hundred degrees higher. Hand stirring is now impossible, but the carbon monoxide gas evolved comes off fast enough to give a reasonable stirring action, and the oxidation of many tons of metal is brought to completion in a matter of five to six hours. A modern large open-hearth furnace will produce a ton of steel every three minutes.

We can now have a look at what Bessemer did. Instead of supplying the oxygen in the form of iron oxide, he decided to use the very simple and cheap method of blowing air through the metal itself. The cost of his oxygen was therefore only that of running his pump. It must have taken a great deal of courage and skill to have tried such a thing with the equipment available at that time. The speed of his process was limited only by the fact that too great an air flow might blow the iron out of the top of his converter. The chemical reaction was now extremely fast, about a thousand times faster than the puddling furnace of that day. Indeed, a modern Bessemer vessel one-tenth the size of a modern open-hearth furnace can convert

eight tons of iron to steel in the three minutes that the open-hearth takes to convert one ton.

We do not have to look far for the cause of this tremendous difference. We now know that at steelmaking temperatures, where the molecules move at high velocities, a good proportion of the collisions between potentially reactive molecules are successful simply because these collisions occur with great force, a force sufficient to overcome the resistance to chemical change caused by the stability of the original molecules. In the language of physical chemistry, a reasonable proportion of the colliding molecules have the required 'energy of activation'. The rates of iron- and steelmaking reactions are therefore governed by the rates at which collisions between the right kind of molecules occur, and this in turn is governed by diffusion and by the rate of mixing. We will see presently, however, that there is at least one exception to this rule.

Henry Bessemer was an engineer, and while spotting that his fuel could be found in the carbon in the iron, he appreciated the importance of mixing, and that was the basic cause of his triumph. He knew that he had to disperse his air in fine bubbles in the molten metal to give a large area of gas–metal interface and to agitate the mixture violently.

However, Bessemer couldn't get out of his difficulties because he was not a physical chemist. One could hardly blame him for that because the breed was not known at that time, despite the extensive hybridisation of scientists that had already taken place.

What went wrong with the Bessemer process immediately after the announcement? The steel made in his experimental work at St. Pancras was perfectly all right, but steel made anywhere else was useless. It was soon found that, by a lucky chance, the pig iron from which the first Bessemer steel was made was very low in phosphorus. The pig iron that other people tried to use contained more phosphorus, and this remained in the final steel and rendered it unworkable.

The laws of nature, together with thermodynamic data, now well known to physical chemists, indicate that oxides of phosphorus are more readily reduced than those of iron. Hence, all of the phosphorus originally in the iron ores comes out in the pig iron from the blast furnace. Selective deoxidation works against the iron-maker here because the phosphorus is deoxidised before or at the same time as the iron. From the very same rules one might deduce that selective *oxidation* cannot help because the iron will oxidise before the phosphorus. Indeed Bessemer blew and blew his phosphorus-containing iron, burning half of it away without making any impression on the phosphorus. He finally gave up trying and confined his process to iron produced from very low-phosphorus ore. Fortunately, there was such an ore in Cumberland, and the process operated successfully there, and it still does until this day. In the United States there are great deposits of low-phosphorus iron ores, and accordingly the process got away to a very good start in that country. Indeed, the great railway expansion that took place nearly a hundred years ago owes itself entirely to the Bessemer process.

It was left to a young chemist with a good scientific training, Sidney Gilchrist Thomas, to find the solution to Bessemer's phosphorus problems twenty years later. I have spoken about selectivity rules governing oxidation and deoxidation as if they were quite simple, but indeed they are not. In recent years a tremendous amount of research work has been done to put these on to a quantitative basis. Let me attempt to outline these rules in modern terms. Every element or compound when present in a phase such as liquid metal has a potential, that is a 'chemical potential'. If an element likes to be in a particular phase, that is it has an attraction for its intimate surroundings, its potential will be low. If, however, it has little liking for its surroundings, then its potential will be relatively high. This chemical potential can be measured by the vapour pressure, p, of the element or

compound, and indeed is given by the quantity, $RT \log p$. Scientists will recognise that this term has the dimensions of work or energy and they know that electrical potential can also be measured in units of work.

In all steelmaking systems we have to deal with three phases, a metal phase, a gaseous phase, and what is probably best simply called a slag phase, the latter consisting of a mixture of inorganic compounds such as silica, lime and iron oxide. An element such as oxygen or phosphorus can be present in all three phases and will have a potential in each of these depending on its concentration and on its attraction for the other elements and compounds which are present. The potential of an element rises logarithmically as its concentration rises, and falls with an increase in attraction, as measured by the heat evolved on entering the phase.

The fundamental second law of thermodynamics tells us that an element can only move on its own from a phase of higher potential to one of lower potential. This is parallel to the simple axiom that water can run only down a hill. We have through this law a means of determining the direction of a chemical reaction. If we want to remove an element such as phosphorus from a metal phase, then we can either raise its potential in that phase or lower its potential in the other phases with which the metal is in contact. We can in fact raise the phosphorus potential in metals but only very slightly, and, indeed, little use can be made of this effect. We know that in practice we cannot lower the potential of phosphorus in the gaseous phase sufficiently. Therefore, we are left with only one thing we can do, and that is to lower the phosphorus potential in the slag phase, and in this lies the secret of the Sidney Gilchrist Thomas process. In simpler language, we must not just attempt to push the phosphorus out of the iron but we must draw it out by means of a slag which has a strong affinity either for phosphorus or for a compound of phosphorus such as P_2O_5. We do not know of any inorganic compounds that have a strong affinity for phosphorus in itself, but only of some which have this for the pentoxide. Strongly basic materials such as lime and magnesia have the affinity that we require.

But we can get P_2O_5 only by oxidation. This means that we need a high oxygen potential in the system in order to help the phosphorus in the metal to go over into the slag. This and a highly basic but fluid slag are the primary conditions for phosphorus removal. Under such conditions the ratio of the phosphorus concentration in the slag to that in the metal, known as the equilibrium partition ratio, can be got up as high as 500 (on a weight basis). At this equilibrium, the phosphorus potentials in the slag and metal are equal and the high relative concentration achieved in the slag is a consequence of the low energy level of the phosphorus atoms because of their high attraction towards their surroundings.

The chemistry of this sounds comparatively simple, but the practical problems facing Thomas of making a basic lining to a Bessemer converter were enormous. Nevertheless he, with the help of his Welsh cousin, Percy Gilchrist, solved them, and the improved process swept the world in an even bigger way than the original Bessemer process. Indeed, it rather took the place of the former. It made possible the development of the great French and German iron and steel industries and the industries of Luxembourg and Belgium. It brought the Bessemer process into use with British home ores in the Cleveland and Midland areas and in South Wales, and went into many other parts of the world. Today over 60 per cent of French steel and one half of the German is made by this process. This British invention allowed the industrial revolution to spread to countries in Europe and brought them into severe competition with this country.

Turning back to the chemistry of the process, we find that in the Thomas-Bessemer converter oxidation of the impurities takes place more or less in sequence. The oxygen of the air first attacks the silicon which is always present to some extent in pig iron, and burns this to silica, the oxide of silicon. When this is mostly gone,

the oxygen then attacks the carbon, and as the quantity present is rather large, some time is taken over this. When the carbon is mostly gone, the air will then oxidise the phosphorus if there is lime there to take up the phosphate produced. Some oxidation of iron also takes place during the phase of phosphorus removal.

Indeed, when one looks at the actual mechanism of this reaction, one sees that the oxygen molecules in the air entering the bottom of the molten-iron bath are much more likely to collide with iron atoms. These form iron oxide which subsequently reacts with the silicon and carbon while the bubbles rise. Oxidation of the phosphorus takes place when the iron oxide gets through to the lime to make a liquid slag on top of the bath, and this can occur only when very little carbon is left in the iron.

However, even after Thomas's success in the removal of phosphorus, the ups and downs of pneumatic processes were far from being at an end because a number of handicaps remained. The air blown through the converter contains 79 per cent of nitrogen, which has a considerable solubility in steel, especially when the carbon is reduced. (Carbon pushes up the potential of nitrogen in the metal phase, that is, opposes its entry.) This solubility is only partially achieved in practice, probably because nitrogen is very stable and only a small proportion of its molecules have the required energy of activation to dissolve in the iron. A certain amount of nitrogen in the steel does not matter for rails or tubes, but, unless special steps are taken to lower it, the steel will not be useful for some other purposes. For example, ships cannot be made indiscriminately of such steel otherwise they might break. The weakness of Bessemer steel in this respect was found to be almost entirely absent in the open-hearth process invented by Siemens and Martin. It was some time before the reason for this difference was discovered: the nitrogen content of open-hearth steel was two to three times less than it was in Bessemer steel.

The open-hearth process also has one very important practical advantage, particularly in Great Britain, in that one can, along with pig iron, melt steel scrap very economically and make fresh good steel thereby. Therefore, even though the open-hearth process goes many times more slowly than the Bessemer process, it nevertheless can be more economical where steels are required for a wide variety of uses.

There is a third handicap to the Thomas and Bessemer processes. Enough heat must be generated to raise the temperature of the metal, melt the slag-forming materials and provide for the high thermal losses in the emitted gases and through the walls of the vessel. This and other operating factors put restrictions on the composition and temperature of the pig iron that can in practice be used; for example, the Thomas process requires an iron high in phosphorus and low in silicon, and the Bessemer process, iron low in phosphorus and high in silicon.

For these reasons, and despite its cheapness and simplicity, the Thomas process steadily gave way to the open-hearth, until today it represents less than 15 per cent of the world's production.

However, the decline is now being arrested, largely through developments made on the Continent. The first of these was the invention by Linde and Fränkl of the regenerator system of producing oxygen from air. This makes possible the large-scale or tonnage production of oxygen and reduces the cost of oxygen gas, but not liquid, by a factor of about five. This has led to a second recent Continental development, the use of this oxygen in the Thomas converter, either by enriching the air with oxygen or by using pure oxygen diluted with steam or with carbon dioxide. The second of these, the oxygen–steam process, was developed in Belgium and is making rapid headway there and also in France and Germany. It is about to do so in this country; there will be two converter plants operating this process by the autumn of this year. In two or three years they will be joined by a third completely new plant.

The use of oxygen in the Thomas–Bessemer process removes most of the difficulties referred to above. First of all the nitrogen content of the steel can be

reduced to very low levels. More important however is that steelmakers are given a new degree of freedom; they are no longer bound to the 20·8 per cent oxygen in air but can change this at will. This allows scrap to be used and temperature–time paths to be followed which are best suited to the particular raw materials and final product. Oxygen brings back the small and frequent batch which is so much easier to integrate into the other continuous processes of a steelworks. It brings its own problems, of course, but these will, I am sure, yield to the research effort now being put to them.

The Bessemer star is in the ascendant again; how far it will rise this time one cannot foretell. It seems likely that we will see a marked increase in the percentage of low-carbon steel made in this country by the process.

One cannot give a discourse on pneumatic steelmaking methods without bringing in a rather healthy young child which was born in Austria. The Thomas and Bessemer processes are almost too fast, and as a consequence it is difficult to control the composition to the required final specification. Part of the trouble is that such a process cannot be stopped without turning the whole vessel on its side, otherwise the steel would run back through the tuyeres at the bottom. It has always been attractive to steelmakers to have a process that can be carried out in a form of ladle, so that it can be stopped at will, samples can be taken and the necessary additions made. The Austrians have achieved this for mild steel by blowing a jet of oxygen down on to the surface of the metal contained in a pear-shaped vessel not unlike a Bessemer converter, but without, of course, the bottom air tuyeres. The impact of the jets sets up a vigorous chemical reaction and provides all the stirring that is necessary. In general, the chemistry of this process is very similar to that of the Thomas process, but with one surprising difference. As I said earlier, in the Thomas process the phosphorus is oxidised into the slag after the carbon is mostly gone. In this top-blowing process, however, the phosphorus comes out together with the carbon or indeed sometimes ahead of it. This has some metallurgical advantages and an interesting chemical explanation.

In top blowing, the ferric oxide formed by the action of the oxygen on the metal no longer has to survive an attack by silicon and carbon but immediately reacts with the lime present on the surface to form a reactive liquid. The slag has an oxygen potential much higher than that of the metal and therefore a reaction takes place between the two. As before, silicon is the first to be oxidised, but the oxidation of carbon is severely handicapped for a number of reasons. First of all the solubility of carbon in slag is effectively zero and the solubility of oxygen in the metal is low, so that carbon–oxygen atom collisions in either phase are infrequent. More important, however, is the fact that the product of combustion, carbon monoxide, has a very low solubility in both phases and effectively exists only as a gas. Because of the surface tension forces it is difficult for bubbles of gas to form in the metal phase or at the slag–metal interface, and because of the low solubility of carbon monoxide, very high supersaturations are necessary before nucleation of bubbles can occur. We have here in top blowing a case of a reaction being slowed down because of the difficulty the products have in getting away. Steelmakers will recognise that similar conditions apply in the open-hearth furnace where iron oxide is added to the slag, but these are not so pronounced as in top blowing.

In contrast with carbon, phosphorus has an easy road in reacting with slag. Practically every atom of phosphorus in the metal which arrives at the slag interface is drawn over into the latter phase by the affinity forces. To affect its oxidation it needs to give up electrons to the slag, and because of the high concentration of ferric iron, which can take up electrons to form ferrous iron, this is very easy.

We have seen earlier that Thomas by introducing lime made it possible to change the order of oxidation of elements and remove phosphorus before burning the iron. Now with top blowing one can make a second change of order and remove

phosphorus before or in parallel with carbon. In comparison with bottom blowing the effect is very large at carbon contents above 0·1 per cent. At 1 per cent carbon the phosphorus partition ratios in top blowing are one thousand times greater than in bottom blowing.

Although this process is so far limited to low-phosphorus irons (below 0·2 per cent), where the slag volume is not too great for the oxygen jet to penetrate, it has a number of *potential* advantages in addition to those given above. Carbon steels can be made directly from medium-phosphorus iron without blowing the iron down to very low carbon levels and re-carburising. Because phosphorus can be removed at lower temperatures where higher partition ratios obtain, the losses of iron to the slag need not be so high.

On its present basis, top blowing is making considerable progress in North America as well as in Europe, and some new developments to deal with the higher phosphorus irons in Great Britain are to be expected. These might well take the form of intermediate processes.

It is of interest to note that these processes of using steam and of top blowing were partly anticipated in Bessemer's patents nearly a hundred years ago.

It would be fitting to conclude this address with a view of the future of pneumatic processes. We have seen that Bessemer's converter is expanding its field through tonnage oxygen and that a new top blowing process is growing up alongside it. But steelmaking will still depend on the intermediate production of the carbon alloy, pig iron, a product which is not only off the direct route from ore to steel but is costly to make because of the special requirements of blast furnaces.

It has been a desire of steelmakers for many generations to develop a direct steelmaking process, that is one which converts the ore to a low-carbon steel directly. A similiar process, the sponge-iron process, has successfully operated in Sweden for some years. This uses carbon monoxide gas to reduce ore at temperatures below 1000° C. where both the iron and the ore product are solid. More recently the Americans have developed the 'H-iron process' (ref. 4) in which high-pressure hydrogen, produced by modern cheap methods from oil or hydrocarbon gas, is used to reduce powdered ore in a fluidised bed at temperatures near 500°C. These processes cannot make steel because the impurities in the ore cannot be separated. They are important however, because of their ability to produce for steel furnaces a good feed material if one starts with a high grade ore.

We have seen that the speed of Bessemer's process came about through the dispersion of air-bubbles in a liquid metal. Similar speed potentialities would exist if we reversed this system and dispersed small droplets of liquid ore in a high-temperature gas. Furthermore, if this gas could be maintained at a low oxygen potential, the ore droplets would be reduced to droplets of liquid metal plus slag. If the oxygen potential is controlled at the correct low value and lime is introduced for the slag, neither silicon, carbon nor phosphorus need come into the metal and thus the droplets would be a form of steel rather than pig iron. Finally it would be necessary to precipitate the droplets out of the gas and allow the metal to separate from the slag by gravity and surface tension.

Laboratory experiments and technical developments in allied fields indicate that all the above are possible. Accordingly the British Iron and Steel Research Association is actively engaged in developing such a process under the name 'Cyclosteel'. This is still in its very early stages and nothing like as advanced as Bessemer's process was one hundred years ago. Furthermore, I suspect that even with all the modern techniques and resources at our disposal, it is going to take rather longer than it took Bessemer to get through the practical problems. It has, however, got away to a promising start and, in the preparatory stages, we have already made small quantities of very crude steel.

The process is best suited for an ore which is already in a finely divided state.

Many sources already exist, particularly as products of ore-beneficiating processes. The ore is to be pre-heated, probably in a fluidised bed to a temperature just below where it becomes sticky, and then injected into the very hot reducing gas. The latter is to be produced by a fuel such as pulverised coal or atomised fuel oil with only sufficient air and oxygen partly to burn it. It is almost certainly necessary to use oxygen to maintain the necessary high temperature and low oxygen potential at one and the same time. From preliminary trials we know that the two reactions, ore plus carbon monoxide going to iron plus slag and carbon dioxide, and coal plus oxygen and carbon dioxide going to carbon monoxide, are fast with finely divided particles and only a short time in the reaction zone seems to be necessary.

The separation of the gas from the precipitated products will take place by gravity and cyclonic centrifugal action, and a settling chamber finally separates the iron from the slag.

The cyclosteel process is in its very early stages and is only referred to here because of the way it fits into the evolution which began with Bessemer. Formidable technical obstacles stand in the way and will have to be surmounted. On the other hand, a method of making steel from powdered ore and coal is extremely attractive, especially as it avoids the sinter plant and the coke ovens and widens the range of fuels that could be used. It is unlikely to replace the blast furnace, at least for many years, but if logic can be converted into practice it will take a place among pneumatic steelmaking processes.

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THE PRESENTATION OF GEOLOGICAL INFORMATION IN MAPS[1]

ADDRESS BY DR. T. ROBERTSON

PRESIDENT OF SECTION C

GEOLOGY depends largely upon where rocks occur on the surface of the earth and within a few miles below that surface. It is therefore necessary to have some method of indicating both where the particular rocks we wish to speak about are to be found and their relationship to one another in space. Setting aside globes and models as specialised and not, in general, practical ways of communicating such information, the geologist, like other scientific workers, is thrown back upon the various methods of projection by means of which man has for many centuries endeavoured to transmit through the medium of two dimensions his ideas regarding the interrelationships of things conceived of as exisiting in three dimensions. It is this essential problem of the communication of geological ideas and results by means of maps that I propose to discuss.

First it is necessary to define what is meant by a geological map. For present purposes I am deliberately confining my remarks for the most part to a map which is a combined lithological, stratigraphical and structural representation of what is known or deduced about the rocks in a region or area of the earth's surface. This leaves aside all special maps designed to bring out such points as the geologist himself wishes to emphasise at the expense of the data that are conventionally considered to be necessary to the conveyance of a balanced representation of lithology, stratigraphy and structure. It is important to remember, however, that there cannot, in the very nature of the case, be one ideal geological map of an area any more than there can be one ideal geographical map. Everything depends upon what kinds of things we wish to show.

For purposes of discussion it is convenient to look upon maps showing the geology of a fully surveyed region as being of three kinds from the point of view of magnitude; first, the detailed map at a scale of 1: 12,000 or larger, showing as far as possible the individual geological observations; second, a general map at a scale of, say, 1:25,000 to 1: 100,000, showing as much of the geology as it is possible to include with clarity and consistency; and, third, an index map at a scale of 1: 250,000 or less, showing only the main regional features.

The reason for such geological maps as I am considering, and the justification for their existence, are not always fully appreciated. I think most people look upon the geological maps produced by the various national surveys as being mainly a method of providing as much information as can be conveniently given in codified form, with the hope that some of it will be useful to anyone who is in search of geological facts for any given purpose, be it scientific or economic. The true geologist will aim higher. Though fully conscious of the limitations imposed, on the one hand through lack of information, on the other through lack of cartographic space, he will endeavour to make his map tell as lucidly as possible the history of the region throughout the continuous succession of geographies that it has experienced. I emphasise the idea of continuity, for, no matter how much of the record is missing the geological life-history of the region is one and indivisible. I know there are those who say they are only surveying the drift, or the Trias, or the Caledonian orogeny or a particular type of intrusion. Blinkers may keep a horse on the road but they keep a geologist in the ditch. It is not thus that geological maps are made.

[1] Address delivered at the Sheffield Meeting of the British Association on September 3, 1956.

General Statement of the Cartographic Problem

A map primarily shows a selection by the cartographer of the following items: (*a*) individual facts regarding the subject with which the map is concerned; (*b*) information and deductions regarding the spatial relationship between such facts; and (*c*) geometrical conventions designed either to show the relationship between such facts and deductions as he has selected or to show their relationship to other relevant facts and conceptions which are generally acknowledged as established.

It is a mistake to think that there is or should necessarily be any real portrayal of Nature in a map. Our human aspirations make us wish to find out as much about Nature as our minds can grasp; but our limitations are such that we cannot make progress except by selecting a small number of natural phenomena, studying them in partial or entire isolation from their natural setting and, on the basis of this study, making deductions and generalisations regarding the branch of science in which we are interested. In the first instance, however, it was we who selected what we thought best for our purpose. If we test our first conclusions by further repeated selection and study we can make progress in our endeavour to apprehend more about Nature, but it is not my present intention to discuss the alternating inductive-deductive flux in research.

Let us apply the considerations stated above to geological mapping and cartography. At the very start of our field investigations we abstract from geology as a whole the subject that we choose to study, and when we study a particular item in that subject we study it first of all in abstraction from the rest of the subject. Having reached conclusions from our studies of abstracted items we fit these conclusions together to make a coherent concretion. Then at a later stage, when we wish to convey to others the final conclusions to which our studies have led us, we abstract from the sum total of our knowledge of the subject such information as we think will best convey our conclusions. Of this we put on the map such abstracted items as we think will convey as much of our message as possible. It is often not clearly appreciated that such processes can only provide us with a very inadequate picture of the Nature that we profess to study.

Now the geological cartographer must express and link his facts and deductions by adopting a series of conventions that permit co-ordination with the conventions used to convey topographical information, otherwise his map has only diagrammatic value, even if it be accurately to scale. Hence the geological map must in general have a topographical base. With certain exceptions it is not only desirable but almost necessary to keep the methods of portrayal of the topographical and the geological data distinct. Much of the success of a geological map, indeed, depends upon a clear distinction between these two sets of data.

The data that may be obtained by a topographical surveyor in the field are endless in variety and number, and a selection must be made of such things as it is decided can be most profitably shown on the map. There is fairly general agreement throughout the world as to what should be shown on various types of maps and, indeed, to a considerable extent there is agreement as to the general methods of showing them. The same situation exists in geological surveying and cartography. What is different is the need to show on the geological map not only things that are seen on the surface but also what is deduced from them regarding what lies beneath the surface. The distinction is primarily not one of degree but of kind. The topographical map is a record of data; the geological map is a summation of conclusions, based upon a record of data which is commonly far too bulky to appear in detail on the map.

When we deal with what lies under the ground the general means of conveying geological information about this is by use of a set of devices applied to what is depicted, either on the basis of observation or deduction, as existing at the surface or immediately below vegetation and superficial deposits.

This, then, is the geologist's cartographic problem; how best to show the following kinds of information on a map: (*a*) the places where the geological evidence is to be found; (*b*) the nature of that evidence; (*c*) the summational relationship of the geological evidence to the topography; and (*d*) deductions regarding the behaviour both laterally and in depth of the rocks and of the structural elements affecting them.

At this point we meet a difficulty. The purpose of the geologist's work is to provide, in such detail as conditions allow or circumstances demand, a description of the character and disposition of the rocks underlying the area being surveyed. This cannot be done, even in the simplest cases, without the production of both a map and a written description. No geological map is a complete scientific study in itself, for it is not a representation of facts alone but a representation of a combined lithological and structural conception based upon the scientific judgment of the surveyor; and this judgment is based upon evidence which ranges from virtual certainty in one area, or regarding one item, to the best guess under circumstances not sensibly removed from ignorance in another. Hence the need for an explanatory account; first, to indicate the degree of reliability to be placed upon the various parts of the map; second, to amplify the necessarily imperfect endeavour made in the map to convey information in three dimensions; and third to expound and justify the scientific conclusions and generalisations reached during the survey. The result of the geological survey of any area is a complex of interwoven research items leading to theoretical and practical deductions which have application not only in that area but throughout the whole range of geology. In their turn the findings in the given area are subject to repercussions from geological research throughout the whole world.

It must be insisted, however, and this is never forgotten by the genuine surveyor as distinct from the research-item specialist, that the map is fundamental. Many a geologist's map has spoken with more conviction than its accompanying descriptive account, and not always in the same sense.

It may be noted here that the stratigraphical table which accompanies, or is incorporated in, most geological maps is properly to be regarded as a specialised and condensed descriptive account of the sequence.

The Design of the Cartographic Scheme

The various points that require to be taken into account in preparing a cartographic scheme are well worth careful consideration by the prospective geological cartographer. I have only space, however, to touch upon a few of them.

A useful critical review of past and present practice in the published geological maps of the various national surveys was given by Professor D. L. Linton at the Dundee meeting of the British Association in 1947.[1] His approach, however, was from the cartographical as distinct from the geological side. One of my chief concerns is that the cartographic knowledge of so many geologists is not sufficient to permit them to express the results of their work properly in maps. Until they possess the necessary understanding to do this they are unable to appreciate the crying need for such codification, standardisation and internationalisation as Professor Linton rightly advocates. The chaotic and absurd situations to which he draws attention appear to me to be adequate proof of the shortcomings in this respect of successive generations of geologists.

The Topographical Base. The position of any geological boundary line and the location of any geological exposure are indicated on the map by reference to the topographical base. It follows that the accuracy of location of the geology cannot be greater than that of the topography. Whatever the faults of the topographical map, however, a geological location referred to the nearest mapped point is still correct in

[1] 'The Ideal Geological Map', *The Advancement of Science*, vol. v, no. 18, July 1948, pp. 141–9.

reference to that point, and this location can be shown on a better issue of the topography when this is available. The position and trend of a geological line are usually adjustable to some extent also, and what appears on an inaccurate map to be a zig-zag course when the line is drawn through a number of points fixed at intervals along its extension may be found to be straight or evenly curved on a corrected edition of the topography. Thus work done on a map containing minor inaccuracies is not usually wasted, as the adjustment called for in 'correction' is often within the limits of accuracy of the geological mapping or at least within the limits of accuracy demanded by the deductions made from the mapping. It is a different matter with larger inaccuracies. In what follows it will be assumed that a satisfactorily accurate topographical base exists, without which, indeed, no useful geological map can be made.

Geological maps accompanying records of research work are often unsatisfactory owing to lack of detail in the topography. The most important immediate question that must arise regarding any piece of evidence is that of its location.

The Geological Outline. A topographical base is necessary, but it is of no use if it obscures the geology. For satisfactory distinction between topographical and geological lines and lettering a difference is needed, either in tone or colour. It is well to remember that if the topographical details are not quite clear they can always be checked with a simple topographical map, but it is either difficult or impossible to check the geology if it is printed in an unassertive way. The chief desideratum in delineation on a geological map is that the geology should be bolder in treatment than the topography, even at the expense of a little loss of precision, as, for example, by the adoption of somewhat thicker lines and a coarser texture for the geology.

Along with the outline the cartographer must consider the ornamental pattern that is usually needed to show the surface distribution of the different rocks when colour is not used. Many sketch-maps are ineffective because of haphazard choice of patterns; and there is much to be said for the suggestion by J. W. Evans that directions of rulings and other pattern devices might be allotted to the different stratigraphical systems, etc. I do not wish to enlarge on this subject, but diagonal lines are almost invariably confusing and should be eschewed. There is at least one Special Report of the Geological Survey of Great Britain in which the ornamentation of nearly every text-figure is effectively carried out by the use of lines parallel to the top of the page.

The Colour Scheme. Even the earliest geological maps had recourse to the device of differences in colour to indicate the distribution of the various lithological and stratigraphical formations. Ornament in self-colour is useless where there is intricacy in structure or in surface distribution, and colour has the advantage over ornament that it need not obscure the topography to nearly the same extent. The question of colour, however, is a very large one and I cannot do it justice here.

There should as far as possible be a colour or tint for each subdivision of the rocks shown on the map; and some generally acknowledged convention in the choice of colours should be adhered to if such a convention exists. The colour conventions in different countries and at different times have varied greatly. A few colours seem always to have had a certain international validity; for example, various shades of green for Cretaceous, yellows and dull browns for Triassic and other sandstones, blues for limestones or formations containing much calcareous material, grey for coal-bearing measures and red for granite.

On the whole the choice of colours should be made with a view to bringing out the relationships and contrasts between the various groups of rocks, remembering always that artistic taste and economy in the use of colours may well go hand in hand.

The Formational Notation. In practice the use of colour alone to indicate the various formations is not found to be entirely satisfactory, for a number of reasons

which it is unnecessary to specify at the moment. It is therefore necessary to assign symbols as well as colours to the different rock-groups. This does not mean merely that each colour has its symbol and vice versa. The symbol may mean something in itself; and it is a pity to lose this opportunity by merely numbering the various colours in the map index from top to bottom or bottom to top.

In many instances the initial letter, or letters, of the formational name are used, for example, SL—Wenlock Limestone, LKS—Lower Keuper Sandstone, Csl—Carboniferous, St. Louis Limestone, Sc.—Silurian, Clinton Formation. This is distinctive and effective within limits. Whatever is said and done about classification the St. Louis Limestone *is* the St. Louis Limestone whatever its age, whatever it may pass into laterally and whatever it may be equivalent to a thousand miles away. This definiteness, however, easily becomes a drawback, for each district gives rise to its own crop of names unless correlation is so simple that the names of the first district examined can be carried through with complete certainty. Even so, the best development of a formation is by no means always in the area examined first, and the result may be a non-typical type locality.

It should also be remembered that the formational name does not convey in itself any enlightenment regarding the position of the formation in the general stratigraphical sequence. Most of those who have occasion to use geological maps are likely to know the successional order of the different systems and possibly also their major divisions; but it is unfair to expect anyone except those really working on the subjects to say exactly whereabouts in the succession the Halesowen Group lies, or the Hoar Edge Grit. It is not enough to say that there is a stratigraphical table at the side of the map. Reference may be easy, but it is a separate and an additional operation. A great amount of energy is consumed unnecessarily in learning the details of the *names* of subdivisions of a sequence. It should never be forgotten that the primary purpose of the map is to convey information as easily as possible. Formational names are necessary to the stratigrapher in his work and in the exposition of his conclusions. It is quite common, however, for the uninitiated reader of a map, be he a geologist or not, to be forced to grasp the meaning of several formational names whereas, for his immediate purpose, it is unnecessary to have any names.

Symbols and contractions on any map should not merely mean something, they should be as full of meaning as possible, for we should try to convey as much information as we can through the medium at our disposal. For this reason one of the most effective devices is that of an algebraic serial notation for formational symbols. That used by the Geological Survey of Great Britain may be cited in illustration since it exploits to the full the possibilities of such a notation. It was first introduced one hundred years ago by Murchison and has gradually been elaborated and modified. Each system has assigned to it a letter, running from '*a*' for Cambrian through the letters of the alphabet to '*l*' for Pleistocene. Index numbers are added to these letters to represent the major subdivisions of the systems, starting with ^{1} at the base of the system. For example, the main divisions of the Carboniferous are numbered from d^1 at the bottom to d^6 at the top. For closer subdivision a second series of indices is added, this time in alphabetical form. Thus d^{6a} is the lowest subdivision of the d^6 division of the Carboniferous. The process can if necessary be carried a stage further by adding another index, this time numerical, if it is thought desirable to subdivide d^{6a}.

Such a scheme automatically indicates the relative positions of formations in the stratigraphical sequence. Whatever f^2 may be *called* it must be a subdivision of the Triassic younger than f^1 and older than f^3; and it must be a formation considered to occupy the same relative position in the sequence as f^2 in any other district—subject, of course, to all the reservations and qualifications so dear to the heart of the stratigrapher and palaeontologist. Quite commonly this is as much as we require

to know about it in reading a map for any given purpose; and usually it is much more important to know this than to know that it is *called* the Bunter Pebble Beds, especially if that formation is non-pebbly in the particular area under consideration.

One of the noteworthy points about this scheme is that it allows for compact indication of various degrees of uncertainty and for the lumping of strata which are not able to be closely classified owing to paucity of evidence. Thus, such notational devices as the following are practically self-explanatory: *ef*, *e–f*, $e–f^{1}$, f^{1-3}, $f^{1,3}$, $?f^{1-4}$, $f^{?1-3}$, $f^{1-?3}$.

The limitations of such a scheme are obvious. Interpolation, for example, is difficult, for nothing can be added elsewhere than at the top of each set of groups without altering the serial numbering of the overlying bed or beds. Thus d^{6} was added above d^{5} (Coal Measures) to represent the highest Carboniferous beds which had previously been grouped with the Permian. If, however, we wish to subdivide d^{2} then it is necessary to resort to a further set of indices such as d^{2a}, d^{2b} and d^{2c}, though each of these groups may be more important than the whole of g^{10}. The whole scheme was strained, but not broken, when the Ordovician was created and the letter *b* had to serve for two systems instead of one, for it would have been unthinkable to have relettered everything except Cambrian. There are other inconsistencies, such as *t* for Torridonian. Still, as stratigraphy is not and may never be a complete subject, a perfect serial notation is a dream for the future internationalist.

The Structural Symbolism.[1] Another important use of symbols on a general geological map is to convey information regarding the structure of the rocks. The use of symbols, indeed, is the only practical direct method we have of indicating how the rocks are disposed below the present surface of the ground. It enables us to envisage the third or vertical dimension without which our knowledge of the area loses 90 per cent. of its usefulness. By indirect means, such as a consideration of the stratal sequence, of the areal distribution of formations and of the relationship of geological boundaries to contours, we may deduce a probable structure in comparatively simple country ; but a structural symbolism is essential to the provision of enough information to enable the geologist to think or speculate profitably in the solid.

In making the map itself we are usually faced with what looks like a jig-saw puzzle from which nine-tenths or more of the pieces have been removed, leaving the remaining pieces lying in their proper positions. From this we must construct what we consider to be as close an approximation to the whole truth as possible by the use of geological deductions based upon the evidence before us. In doing this the areal distribution of formations or other rock units is the primary requisite, for without it the map cannot exist. By inspection of the combined primary (or observed) and secondary (or deduced) series of outcrops on a map a considerable amount of information can be obtained regarding the structure, but not by any means everything; for the total structural scheme is not usually fully defined in the process of obtaining the secondary series of outcrops. How, then, can further known and essential structural information be inserted? Not usually by a pattern or colour symbolism, for these are already fully occupied in showing the distribution of rocks and formations. Besides, an all-over symbolism almost invariably commits us to much more regarding the structure than we should ever be justified in publishing on a map. Another difficulty is that our structural information is derived from heterogeneous data referring to all the different groups of rocks in the district. We may, of course, have a fully authenticated structural representation of a given coal seam or other bed over much of the area, based on accurate survey-

[1] Much of the substance of this section is contained in T. Robertson, ' Practical use of geology in coal mining ', *Trans. Inst. Mining Surveyors*, vol. xxix, pt. iv, Oct. 1949, pp. 129–63.

ing and levelling; but that cannot generally be incorporated in a map showing the geology of the area as a whole.

For specific representation of structural data on the geological map, then, the only thing that can be used is a symbolism referring to points instead of to areas. Such point symbolism has two inescapable limitations; first, it must refer to actual points at which evidence is obtainable or it becomes hopelessly confounded with a pattern symbolism and loses its significance; second, the symbol, in contrast with the boundary line, fault, outcrop or marginal limit, is outside the place to which it refers and occupies an appreciable space on the map surface, sometimes interfering with other geological items. The limits thus imposed on structural representation are severe. For example, there may be positive knowledge that the dip of strata must be in a certain direction over a large area, but the only observed dip may be a high one in another direction and indicating the existence of a local ruck, or of the edge of the area of low dip.

Again, normal experience in reducing results from the field map to the general map is that most of the lines can be inserted but that structural symbols and notes have to be reduced so drastically because of space considerations that the map is in danger of becoming unbalanced in the proportions of the information conveyed regarding sequence and structure.

An important deduction from what has just been said is that only the geologist can select what is to be inserted on the general map. Two opposing tendencies must be balanced. The general map at a scale of, say, 1 : 25,000 to 1 : 100,000 is not fundamentally a generalised map. It is prepared because its scale is such that adequate detail may be inserted whilst allowing of easy simultaneous ocular grasp embracing localities a considerable distance apart and so bringing general relationships into as clear a light as possible. Thus there must be as little sacrifice of detail as is consistent with clarity. This is comparatively easy on the areal distribution side, but on the structural side it is usually a matter for compromise. Take, as a simple example, a block of strata in which the dip at the surface gradually and progressively increases in a given direction from 5° to 45°. There may be room in this block as represented on the general map for six dip-arrows, say two at 5°, two at 20° or 25° and two at 45°. The inadequacy of this to meet the case is obvious; and it is usually impossible to indicate whether the dips shown are the only ones observed and the structure is deduced from them alone, or whether they are selected from abundant evidence in order to convey the best possible idea of the facts by means of what can be inserted upon the map. This being so in a simple case, it is easy to imagine the degree of inadequacy of structural symbolism in a region of even slight complexity.

The Quality of the Cartographical Material

Since the cartographic scheme is applied to the data obtained in the field it is necessary for the surveyor to have his information in suitable form for presentation. In practice this means that certain principles underlying the formulation of the conclusions derived from the field work require consideration.

A geological map was defined earlier as a combined lithological, stratigraphical and structural representation of a region of the earth's surface. Here the interesting question arises as to how far it is possible to produce a satisfactory map without any theory. The earlier geological maps, including those of the official survey of this country, did not go much beyond the purely lithological stage, indicating where the different kinds of rocks then recognised were to be seen at the surface. Theory, or at least the application of theoretical principles, was not, and indeed could not be non-existent but it was reduced to its lowest possible terms.

It is, nevertheless, generally true that any map claiming to make an appreciable contribution to the advancement of the science of geology must be based upon a

theory regarding the succession and structure of the rocks. This being so, two further questions arise: What is the character of the accuracy of the data upon which deductions and conclusions are based? and What is the nature of the deductive processes that have been applied to these data, and how far have these processes been carried?

There are two different kinds of accuracy to be considered. First, we have the possible accuracy of representation upon the map of geological data which are actually visible; second, the possible accuracy in position of the unseen boundary lines necessarily drawn because of the evidence or because of the demands of consistency in stratigraphical or structural representation.

In the first type of accuracy there are certain situations in reference to the topography shown on the map, such as the corner of a house, the side of a road, or the rock upon which a bench-mark is cut, which can be located with precision. In the more general case, however, positions demanding measured distances from certain topographical points are required. The measurements may be made with precision, and noted down in figures. This is desirable, or even necessary, in some cases, but more often it is the relative position with regard to a geological line or lines already drawn upon the map that is required. In this case the only practicable method is to plot directly on the map. Hence in general the actual location of outcrops, etc., on a field map, even in well-exposed ground, is subject to a possible error of something under 15 ft.—say 10 ft. on the average—if the work is done to the scale of 1 : 10,000. In the end, however, there are various considerations which make it impossible to rely on an accuracy of position in the finished and published work to that scale closer than about 0·02 in. or, say, 20 ft. For this reason it is often desirable to preserve accurate measurements relating to the positions of important points, and even to insert them on the map.

Consider now the other type of accuracy; namely the possible precision in position of the unseen boundary lines, etc. Here we have a possible error varying from a few yards to half a mile or more. It is in a different category from the error in representation, being an error in estimation of an unseen position. In the case of the outcrop of a coal which is known to lie immediately below an exposed and identified marine-bed the position is certain to within a few yards. In the case of a boundary line, represented, say, by a limestone separating two formations and which must therefore lie somewhere between the known top of the overlying group and the known bottom of the underlying one, the possible error is considerable. Especially is this the case if both groups are, say, over 1000 ft. in combined thickness and are subject to folding and faulting. In such circumstances the possible error may easily exceed half a mile. Another factor making for uncertainty is present in areas covered by superficial deposits. It is rarely possible to be certain of the variations in thickness of such deposits, and an outcrop of a gently dipping seam as estimated by assuming a thickness of 20 ft. of drift may be out by 100 yd. or more if the actual thickness of drift is 40 ft.

Eventually the map consists of a series of lines and points the accuracy of location of which varies from complete exactitude to a possible error of a quarter of a mile or more. There is also a variation in the definiteness of direction of the lines from certainty within 1° of arc to uncertainty to the amount of nearly 90°. This angular uncertainty is a common occurrence in the case of faults and must be carefully borne in mind.

The question of degrees in the process of deduction also needs consideration.[1] If we are certain of the occurrence and position of a bed or fault or other boundary line it can be inserted on the map without conjecture as in Figure 1. If we know that another bed occurs in the sequence in the same area at a certain

[1] T. Robertson, ' Practical use of geology in coal mining ', *Trans. Inst. Mining Surveyors*, vol. xxix, pt. iv, Oct. 1949, pp. 129–63.

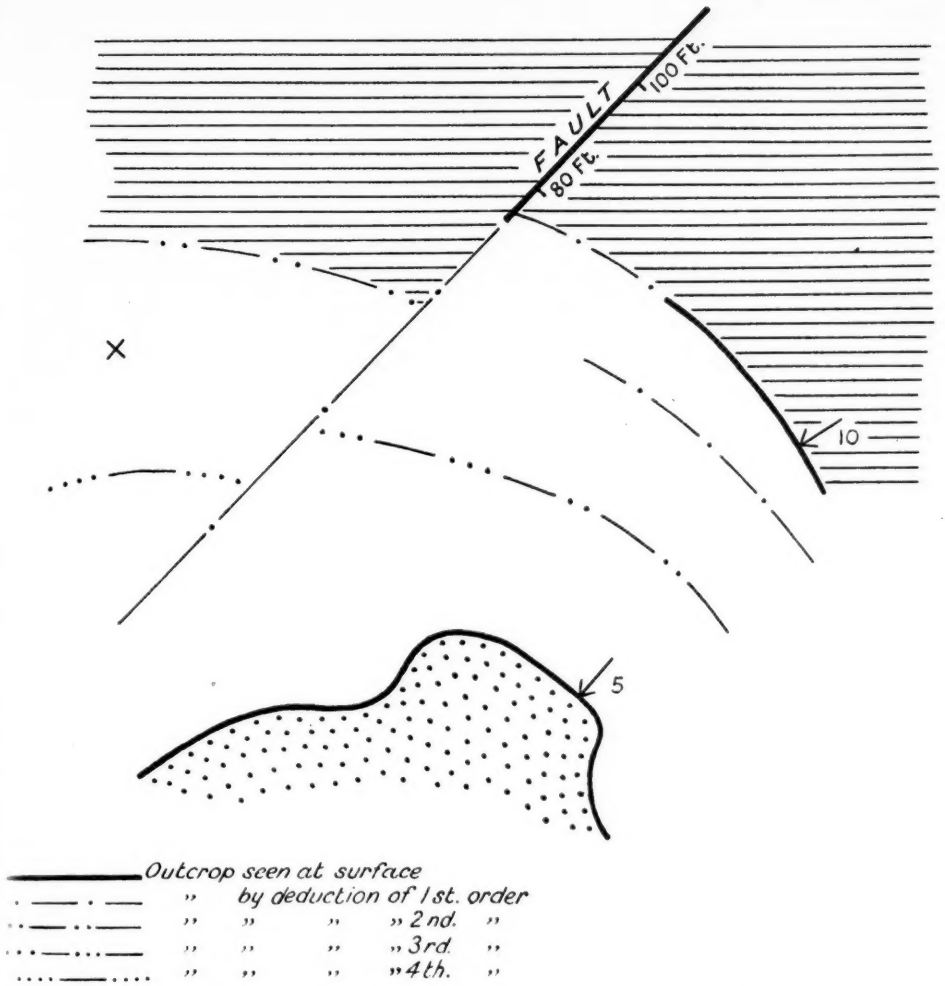

Fig. 1.—Diagram showing orders of deduction used in the construction of a geological map.

distance above the first, and the general lie of the rocks is known, we may insert a deduced position for this second bed based upon the position of the first. This may be called a deduction of the first order. If next we proceed to deduce, from the position of this second bed, the position of a third bed of which the relationship to the second is known but the relationship to the first is not directly known, we make a deduction of the second order. Should we continue, as we often do, the line of outcrop of this third bed according to a plan based upon extrapolation or interpolation from an observed structural scheme, our conclusion regarding its position is a deduction of the third order. If, however, there is any likelihood that lateral variation in sequence or any structural modification may have come into play meantime, then our last deduction regarding the bed in question is one of the fourth order. It can thus be seen how easy it is to build deduction upon deduction until an inverted pyramid of formidable size is balanced upon a very small base. This is a common-place in the more theoretical branches of geology and is not necessarily unprofitable. Frequently, however, the geologist is quite unaware to what degree he has gone in deduction.

In the construction of the map conjecture should be kept to the lowest degree consistent with proper indication of the results obtained. For example, a coalfield

map showing every coal, sandstone, shell-bed and marine horizon that can be squeezed into it is seldom a real indication of the geologist's lack of omniscience on the subject. In this there is unsatisfactoriness through excessive deduction. At the other extreme a trough or dome may be referred to in the descriptive memoir accompanying the map as definitely known to exist, whilst no indication of that fact is shown on the map, because the evidence on which its existence is based is not of such a kind as can be indicated without deduction of the third or fourth order. In this there is unsatisfactoriness through lack of deduction.

Degrees in deduction are not to be confused with amounts of probability. A deduction of the second order is necessarily less probable than the deduction of the first order upon which it is based, but it does not inevitably follow that any deduction of the second order is inherently less probable than one of the first order. So much depends at every stage upon the judgment of the surveyor that a wild guess or a shrewd estimate may alter the balance of probabilities at any time. Nevertheless it is necessary in practice to have some standard of reference; and a full

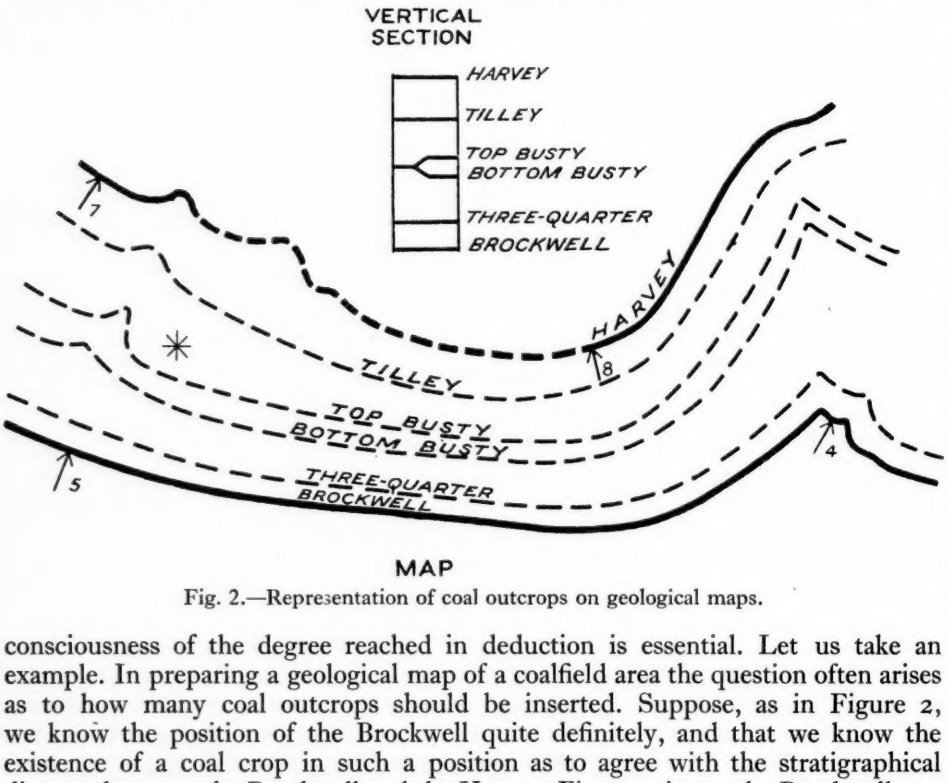

Fig. 2.—Representation of coal outcrops on geological maps.

consciousness of the degree reached in deduction is essential. Let us take an example. In preparing a geological map of a coalfield area the question often arises as to how many coal outcrops should be inserted. Suppose, as in Figure 2, we know the position of the Brockwell quite definitely, and that we know the existence of a coal crop in such a position as to agree with the stratigraphical distance between the Brockwell and the Harvey. First, we insert the Brockwell as a continuous line. Then we insert the Harvey, as a solid line where it is definitely known, but as a broken line where its position is deduced, not proved. The next question is whether to insert the Busty outcrop or not, deducing its position as between the Brockwell and the Harvey; and along with that we have to decide whether to put the Busty as one seam or two. If we do put it in there seems no good reason why we should not also add the Tilley and the Three-Quarter, for once we have the Busty the positions of the other two cannot be very far out. This gives a nice, complete and very plausible sequence which stands a good chance of being near the truth. Suppose, however, a later borehole proves that the outcrop of the Bottom Busty is where shown in the figure by the asterisk. That upsets the whole scheme, for nobody but the geologist who made the map knows how much of the

detailed interpolation to scrap. If, however, the geological map only shows the seams for which there is good evidence, namely the Brockwell and the Harvey, then we are on clearer ground, for the intermediate seam positions are mere interpolation which we ourselves can do quite as well as the surveyor before deciding where to put the borehole to prove the Busty crop; and when we have proved it we know what to scrap.

It should be noted that the estimated distances apart of the various intermediate seams in the figure take into account four different and independent factors, namely, the distances between the seams in the sequence, variations in ground level, the amount and direction of dip, and the thickness of superficial deposits covering the solid rocks.

I have used a simple stratigraphical example, but the controversial literature dealing, for example, with the Scottish Highlands, abounds in examples of the most complex figure-skating upon very thin ice.

Concluding Remarks

I have addressed you on the problems of geological cartography because it is a matter requiring the serious attention of all geologists. A great deal of good published work is lacking in power because it is not supported by adequate maps, and quite often a geologist misses a vital point in his research through being unaware of the full implications contained in his own maps. Attention has necessarily been focused on one particular kind of map, but my comments apply with full force to every kind of cartographic representation of geological information. I hope I have made my main conclusion clear; namely that geological cartography is not a mere tool in the geologist's hand, but an essential part of his field research.

It follows from what has been said that geological surveying and cartography cannot be separated; indeed it is a matter of simple experience that the geological surveyor cannot hand over his results to a cartographer for representation upon a map in any sense similar to that in which this can be done in topographical surveying. In topographical surveying we decide first of all what items we are going to observe, measure and record, and we work accordingly. This is roughly what is meant by saying that the main principle to be observed in topographical surveying is to work from the whole to the part, not from the part to the whole. In geological surveying this is not so. As has often been pointed out, the field geologist does not know what Nature has in store for him when he starts work. The scheme of operations only evolves as he goes forward. He has throughout to be an investigator in a branch of scientific research which is continuously opening out before him. We have thus a condominium in which work from the part to the whole and from the whole to the part must share control. It is, indeed, simply the alternating flux of induction and deduction necessary in any scientific investigation. Only when results are being prepared for publication is the dominant principle that of working from whole to part or general to particular.

It is some hundred years since geological cartography in this country reached the stage in development at which it first became clearly necessary to codify, standardise and regulate its methods of presentation if real progress were to be made in the general communication of geological information and ideas by means of maps. There is a very pregnant sentence in the Preface to De la Beche's *The Geological Observer*. He says '[The object of this book] is to afford a general view of the chief points of [the science of geology] such as existing observations would lead us to infer were established; to show how the correctness of such observations may be tested; and to sketch the directions in which they may apparently be extended'. Note carefully the last clause. If the object of the field geologist in constructing his map had always been similar to that of De la Beche in writing his book, we should have travelled farther in geology and in successful application of its principles in the hundred years since he wrote.

SOME IMPLICATIONS OF THE STUDY OF ANIMAL BEHAVIOUR[1]

ADDRESS BY DR. W. H. THORPE, F.R.S.

PRESIDENT OF SECTION D

THE modern school of animal behaviour studies can, with some truth, be said to have begun with Darwin, for the publication—in 1872—of *The Expression of the Emotions in Man and Animals* was an important landmark and ultimately had a big influence. But its immediate effects were small, as were those of Spalding's admirable experiments on instinctive behaviour in young chicks, published the same year; and the subject remained largely in the anecdotal phase until the later years of the century. Then the rise of the associationist or connexionist psychology in America, based on Thorndike's experiments with the 'trial-and-error' escape of cats from his puzzle-boxes, and the development in Russia of the study of the conditioned reflex, led to a purely mechanistic-physiological view of animal behaviour which found expression in the 'Behaviourism' of Watson and Loeb. Behaviourism had its merits, and was in some degree a healthy reaction from the uncritical anecdotalism of earlier writers. But it was woefully inadequate as a general theory of animal behaviour, and its life, in its original rather crude form, was short. In its turn it gave rise to revulsion on the part of those who were studying the more complex aspects of behaviour, such as visual perception, where the inadequacies of behaviourism and simple associationist ideas were startlingly apparent. So it was that a group of German psychologists and sensory physiologists—Wertheimer, Köhler and Koffka—gave the pendulum a swing in the other direction with the development of the gestalt psychology.

What was happening in England while the pendulum was thus oscillating in other parts of the world? The story of English work in animal behaviour is, I think, very characteristic of the English mentality. There was a cautious, conservative unwillingness to be rushed from one extreme to another, which can be found in so many branches of English life and thought. Spalding had unfortunately died young, and the characteristically English students of animal behaviour succeeding him were Lloyd Morgan and Hobhouse. These two had indeed reacted from the uncritical credulity of Romanes but not with any extreme swing in the other direction; and I think it is instructive to look at the animal through their spectacles. Both Lloyd Morgan and Hobhouse were primarily philosophers, and their deservedly great reputation rests principally on their philosophical work. But they were both men of great general learning, having a sympathetic understanding of animals, combined with a genuine interest in experiment. While giving full weight to field observation and anecdote where reliable, they both saw that the urgent problems could only be solved by experimental investigation. It would have been particularly appropriate had this address come a year earlier, since Lloyd Morgan, who was the first president of Section J (Edinburgh 1921), held the chair of geology and zoology and later psychology and education, at Bristol. I intend to take him as the representative of the two and compare his attitude to animal nature with our own, more than half a century later.

Lloyd Morgan was the real founder of experimental animal psychology. His attitude to animal nature can best be made clear by considering his belief in the three levels of mind (assuming that the instinctive was a level of mind and not only a level of body behaviour). These three levels he called percipient, perceptive and

[1] Address delivered at the Sheffield Meeting of the British Association on August 30, 1956.

reflective, and he regarded them as associated respectively with the subconscious, conscious and self-conscious mind. Percipience he regarded as the equivalent of sense awareness; animals having nothing more than sense awareness would be percipient, as babies at birth or perhaps only before birth. He says: 'Bottom-level minds, if such there be, are percipient only, having sense awareness and no more. Middle-level minds are perceptive and percipient, while top-level minds are reflective, perceptive and percipient.' Thus, in contrast to Sherrington (1906), who spoke of instinct as implying some slight degree of anticipation, Lloyd Morgan regards the subconscious mind of instinct as mind at the percipient level of mental development, having no sense of past or future. Conscious mind is perceptive, and this betokens expectation based *on what has been learned on prior occasions*. The reflective mind is regarded as self-conscious in the sense of embracing, to some degree at least, both past and future in relation to an awareness of self. So he would see in 'trial-and-error' (an expression which, incidentally, we owe to him) nothing more than unreflective 'fore-experience' or simple expectation of the future without a fore-plan for it; whereas trial-and-error when it becomes reflective leads to a plan for the future also. This 'plan' is what W. Köhler calls 'insight'. The 'plan' may be too elementary or too incomplete to cope with a given situation; in which case the only course open to the animal is to continue with trial-and-error, at any rate for the time being. But if the external situation is very simple or if the plan becomes sufficiently articulate and complex, then it may give rise to the phenomenon known as latent learning or insight learning and so the plan becomes open to experimental investigation. Lloyd Morgan believed emphatically that the study of animal behaviour can give the investigator real knowledge of the nature of mental processes in animals, and that these mental processes play their part in controlling behaviour. These mental events he regarded as something different from physical events. In this he was a psychologist of the old school, making quite clear his disagreement with any type of theory which looked upon animal behaviour as explicable in terms of physiological mechanism and nothing more. Yet it is instructive to remember that Lloyd Morgan (1894), far from beating swords into ploughshares, successfully beat Occam's Razor into a Canon and thereby provided the Behaviourists with their chief weapon. That Lloyd Morgan used it himself with great effect combined with circumspection hardly needs saying. Whether the Behaviourists used it justifiably or wisely is another matter; it has certainly been of enormous value to comparative behaviour studies in general and indeed in many other fields of science also.

This question of the relation between mechanism and mental events is one which has again come strongly to the fore as a result of the development of electronic machines which are not only conditionable but which can even display behaviour superficially characteristic of latent learning and of insight learning; and of course the postulation and investigation of mechanism is an essential method of approach for biologists. But that does not mean that the mechanistic hypothesis is conceivable as a fundamental explanatory principle in a biological philosophy, and it was this that Lloyd Morgan was seeking. As Woodger has pointed out, we may talk of purposes in animals as in human beings, and we may, and nearly all of us do, think that something like purpose—at any rate in some animals—is the cause of at least the 'higher' types of learning. But it is extremely difficult, at the levels with which we normally deal as biologists, to prove the causal effectiveness of purpose in the sense that all other possible explanations are excluded. It is always possible to defend microscopic mechanism in principle, if you wish to do so, by making your mechanism complicated enough and by postulating enough sub-mechanisms to meet all contingencies. This game of making hypothetical sub-mechanisms can be carried on a long way and is at first sight very convincing, although there is ultimately a limit to it. For if we go on making our supposed mechanisms smaller and smaller, we come eventually to the sub-atomic level at which the concepts of

form and even the idea of occupation of space lose all meaning, and the idea of physical causation has likewise disappeared. That, however, is fortunately far beyond the level with which biologists have, as yet, to concern themselves; and the level of micro-mechanisms which biologists are tempted to regard as plausible can often be neither refuted nor verified. But, as Agar says, anyone who accepts this kind of explanation as by hypothesis responsible for the whole of animal behaviour, must also of course accept it for his own consciously purposed acts; unless he holds dogmatically that there are factors in human behaviour which are different not only in degree but in kind from the factors determining the acts of lower organisms. Nevertheless, as biologists we are entitled to claim that when we speak of animal behaviour as appearing, shall we say, expectant or anticipatory, we are, for the time being, speaking of a mechanism which acts as if it were expectant or anticipatory, and reserving judgment on more fundamental issues. Biologists may be justified in this attitude only as long as they realise that this is a linguistic device and not a philosophical position. Thus Lloyd Morgan, by avoiding the extreme views of both those camps which were warring on the other side of the Channel and across the Atlantic, achieved a general attitude towards animal behaviour and experiment thereon which is striking for its sanity and balance. Study of it can yield much profit today.

But let us return to consideration of the position on the Continent. The coming of the school of behaviour study to which the name 'Ethology' was first applied in 1935 was an expression of the reaction of zoologists against two extreme camps; those of the old anecdotal comparative psychology on the one hand and the rigid Russian reflexology on the other. The old subjective animal psychology was in danger of having us suppose, for instance, that a bird building its nest for the first time foresees the young it will be feeding some weeks later; that the salmon returning to the river of its birth is looking forward to the spawning period. So it was a natural and indeed an inevitable step for naturalists to get back once again to the descriptive empirical objective phase, and to recommence by observing, recording and analysing the animal's behaviour experimentally—first in as natural a situation as possible, and then with simple experimental controls. And here we see how Darwin was in fact a forerunner of modern ethology, for in his *Expression of the Emotions in Man and Animals* he is concerned not so much with the emotions as they are supposed to be felt by the animal (though this is implied) but with the similarities of emotional expression and behaviour in widely separated groups of animals.

What then was new in the approach to animal behaviour started, and to a large extent characterised, by the early papers of Lorenz? First as to technique: as complete an inventory as possible must be made of the whole system of actions of the animal in its normal environment; and such special techniques as have been developed are primarily those which aid and make more precise this primary observation—the technique of keeping animals under observation yet with the minimum of interference, the technique of marking individuals so that they can be recognised throughout large stretches of their lives, and the techniques which aid the recording of their consequent actions and behaviour. Ethology is thus essentially a naturalist's approach, and it first emphasised and brought under experimental study stereotyped relatively rigid species-characteristic actions (e.g. the web-spinning of spiders, the nest-building of solitary and social wasps). Secondly it drew attention to the evidence that such actions are governed in some degree by an internal drive, a point which most zoologists who failed to remain field naturalists (that is, most zoologists!) seem almost completely to have forgotten. Thirdly it produced an hypothesis to account for this internal drive which allowed scope for the action of hormones, and elaborated it into a hierarchical scheme of organisation.

This internal drive is of the very essence of the meaning of the word instinct.

Most of the workers who had seriously considered the subject prior to 1935 uttered vague generalities about 'tension', 'nervous energy', etc., which were little use either as explanatory concepts or as guides for future research. Konrad Lorenz, in papers which appeared between 1935 and 1939, and Karl Lashley, in a paper dated 1938, realised independently that the more promising way of tackling this problem of the internal drive behind instinctive behaviour would be to study not actually the drive itself, but rather the internal system of co-ordination of special behaviour patterns on the one hand, and the special sensory mechanisms on the other. They saw in fact that the complex and stereotyped action-system and its co-ordinating mechanisms might be more fundamental to the subject than the question of drive, and they suggested that a study of these mechanisms might eventually throw further light on the problems of drive and motivation. Such an approach was not new, for human psychologists (notably Woodworth in 1918) had pointed out that habit mechanisms, at any rate in human beings, may become drives, and had implied that habit and instinct had therefore much in common; and that in both cases the co-ordinating mechanism of the behaviour pattern might itself be producing the drive. One has only to extend this concept from the learned behaviour, which in human beings we call habits, to the inborn instinctive actions of animals to arrive at the first essential idea of the Lorenz-Lashley scheme. Looked at physiologically in this way, drives are seen to be nothing more than expressions of the activity of specific mechanisms, and the general theory which Lorenz built on this quickly provided a more comprehensive framework for the description and further study of instinctive behaviour than any which had preceded it.

The new approach resolved many of the earlier difficulties. It emphasised that there are some elements of what we call instinctive behaviour which are extremely rigid and indeed in their way as constant as anatomical structures, and therefore potentially just as valuable for the study of evolutionary relationships. It was also pointed out by Lorenz that where, as is so often the case, such very rigid action-patterns constitute an end-point or climax of either a major or a minor chain of instinctive behaviour, they are in some sense consummatory in that they effect the release of the tension which has up to that point been activating the instinct in question. Such *consummatory acts* are familiar to all. One need only mention the elaborate nesting and egg-laying behaviour of the solitary wasps, the stereotyped sexual displays of birds, or the elaborate and apparently inborn movements of innumerable predatory animals, whether fish, birds or invertebrates, by which a particular kind of prey is successfully caught. This view, then, involves the idea of the co-ordination mechanism of the fixed consummatory act, giving rise to the tension which activates the instinct, until it finds relief in discharge of the act in the appropriate situation. As long as the act remains undischarged, the animals continue to show an introductory phase comprising more or less complex searching activities, the appetitive behaviour. This behaviour is such that there is a reasonably good chance of it bringing the animal before long into the appropriate situation for the consummatory act to be performed. The animal is *apparently* looking for its prey, *apparently* searching for a nest site or nesting materials; or else is going through a series of actions, such as those employed in nest-making in insects and birds, which will in due course lead on to the completion of the particular chain of instinctive behaviour. This distinction into appetitive behaviour and consummatory act was not in itself new. Sherrington and others had used much the same terms in much the same sense; but it was important just at this juncture because it resolved the apparent disagreement of previous workers as to the relative flexibility and rigidity of instinct. For by definition the consummatory act now became the characteristically fixed part of the behaviour—the fixed-action-pattern, as it is now called—and the preliminary or searching behaviour was regarded as the flexible part where learning ability and perhaps 'intelligence' was able to show itself.

Another immediate advantage of this scheme lay in its apparent avoidance of the vitalistic approach, for the objective of the animals is now no longer seen as the attainment of the final biological goal or result (which it is, of course, inconceivable in most cases, that it could know about), but rather the performance of the consummatory act itself, which can only be achieved as a result of finding the appropriate environmental pattern of stimulation, the *Releaser*, which can 'unlock' the *Innate Releasive Mechanism* and so allow the act to proceed. The new theory substituted a physiologically reasonable system for a largely untenable vitalistic one, and this was another reason why it has ever since yielded such good dividends in promoting research.

While the idea of a relatively rigid consummatory act is thus a theoretically sound one, and while the separation into two categories probably continues to be as useful now as it has ever been, recent researches—particularly those of the last decade or so—have made it abundantly clear that the distinction is not hard and fast. For instance, much appetitive behaviour contains rigid consummatory acts, of what might be termed a minor grade, and many examples are now known in which appetitive behaviour and consummatory act vary greatly in degree of rigidity and flexibility. Both may show some evidence of internal activation; both may be stereotyped to some degree and show some rigidity, and both may be to some extent flexible. I shall return to this subject shortly and give some examples to illustrate the point: suffice it to say now that the characteristic instances of flexible appetitive behaviour and rigid consummatory act can be regarded as the two ends of a series. So to some extent the original picture drawn by Lorenz, which was so attractive in its clarity and simplicity, has, as always seems to happen in scientific research, been complicated and somewhat blurred by the detail since added to the picture. And this in its turn has important implications for our ideas of animal nature. If the internal organisation of the consummatory act is not now seen to be as rigid and stereotyped as was formerly thought, it becomes harder to envisage it as providing the specific internal motivation for behaviour which Lorenz supposed. Besides 'seeking' to discharge its fixed-action-patterns and so relieve the instinctive 'tension' accumulating around the mechanism, there is now evidence that some of the searching is an attempt to find a key environmental situation. Perhaps there is also some real though very limited element of purposiveness in the search for a satisfying situation. Actions are not always brought to cessation by exhaustion of internal drives or by the cessation of the eliciting stimulus. On the contrary, there are external situations which instead of, or in addition to, acting as releasers for the next phase of behaviour, serve to bring the previous phase to a close. In this case they must be effective in stopping a piece of behaviour even when the original eliciting stimulus is still present. They are thus *consummatory stimuli* or consummatory situations; or in simple English, *goals*. Thus we now see that the goal is not always the performance of an action; it may sometimes be the perception of an environmental situation. The attainment of such a situation may in fact be as effective in bringing a particular piece of behaviour to a close as is the satisfaction of a physiological need or exhaustion of the internal drive behind the fixed-action-pattern or consummatory act.

The original instinct theory of Lorenz thus gave a picture of the animal as 'searching', by means of a series of stereotyped orientation mechanisms, for the situations which would release the consummatory acts. The recognition of these situations was largely inborn, though of course it could be modified to a greater or lesser extent by reflex conditioning or other forms of learning, and the act itself was the true goal of the behaviour. When the act is at last performed or, as we would now add, the consummatory situation at last reached, that particular behaviour chain is for the time being completed, and so the episode is brought to a close. It may be a final conclusion, as in such behaviour as the cocoon-spinning of

lepidoptera, where once the highly elaborate series of acts has been accomplished a part of the life cycle has been achieved once and for all and will never be repeated by the individual; or, on the other hand, it may be a type of behaviour which is repeated constantly during the whole life—as feeding or mating, for instance—and the whole system may be due for reactivation in a period of minutes, hours or days by hormonal control internally aroused, or other internal or external complexes of stimulation. We know that, valuable as is this picture, the account it gives is not complete.

These newer theories of animal behaviour have also done valuable service in emphasising the significance of the interaction of learning and instinct in animals. I said just now that learning abilities play their part particularly in the flexible appetitive behaviour. Very many higher animals can learn to adapt their hunting methods, at least in some degree, to the particular circumstances which they encounter, and to the particular methods of avoiding action employed by the prey which they find. In contrast it may be very difficult or impossible for an animal to adapt its cocoon-building or mating behaviour even after repeated lack of success. But if the two categories of appetitive behaviour and consummatory act are not as sharply defined as we thought, then we must be prepared to find (as indeed we do find) that some appetitive behaviour is more rigid than we should have originally expected and some consummatory acts more flexible. And this conclusion has brought into prominence the importance of the type of learning known as imprinting—not really a new category of learning, but merely learning ability restricted to a particular and perhaps very short period of the animal's life and orientated by innate predisposition to a particular circumstance or in a particular direction; as when an insect or an intertidal mollusc learns a particular diurnal rhythm of behaviour from its first day's experience of it, or when a young bird learns the characteristic appearance of the species by following its own parent or learns the niceties of its specific song by imitating its neighbours during its first few days of singing in a territory. This kind of learning is important when we are thinking of the implications of modern behaviour studies for our views on animal nature. Finally, it has been shown that highly elaborate learning processes may be displayed by some animals possessing nervous centres far inferior in mass and complexity of structure to the mammalian cortex. Not so long ago, for instance, insight and insight learning were assumed to be the prerogative of the highest mammals only, and it was thought that only nervous structures of the complexity, mass and elaboration of the mammalian cortex could possibly provide the mechanisms necessary for the production of such apparently intelligent responses. Now we find that behaviour giving good evidence of insight is found not merely in mammals, but also in birds, fish, some arthropods, and perhaps in organisms even lower in the zoological scale. In fact our present knowledge of complexities of perceptual organisation in the homing bird and the foraging honey-bee have made it clear that in certain limited respects some of these creatures rival all but the highest mammals, if indeed they are inferior even to them. So it follows that our ideas as to the mechanism of brain action may need a drastic revision as a result of some of the recent studies of sub-mammalian and invertebrate behaviour.

Let us now consider actual examples of recent research bearing upon some of these crucial questions of the goal of the animal, the perception of complex releasers and the level of integration achieved. First let it be said that it is quite difficult to find an example of a metazoan animal whose sense organs and responses are such that one is convinced that it is, to use Lloyd Morgan's phrase, percipient only—that is, responding to crude sense-data and not in fact displaying some more or less elaborate organisation of perceptual relations similar to what we normally regard as insight. Until a few years ago the division of labour among workers of the honey-bee was thought to be based upon the physiological states of the insects

concerned, and not to be directly correlated with the needs of the colony as a whole. Thus it was thought that the newly-emerged worker was compelled, by physiological conditions arising from the age and state of its organs of internal secretion and its nutrition, to go through the series of actions appropriate to the cleaning out and preparation of cells in the hive, whether this particular work was required by the hive at that time or not. If it was unable to find a place for this particular activity, then it was forced to remain idle until the development of its glandular mechanisms rendered it capable of the next series of activities, which might, for instance, be comb-building. So it was thought that each individual went through an inevitable and ordered series of actions, such that by the time the first half of its life was completed it would have done its share of all the duties requiring to be done inside the hive, its final occupation (marking the end of its life as an indoor bee) being that of acting as a guard bee at the entrance. After that, it becomes an outdoor bee, and a similar and further series of actions appropriate to the outdoor life, such as foraging, are then gone through, physiologically conditioned as before. While it may be that the *average* worker bee may spend the greater part of her time in some such sequence of actions, it is now clear, from the work of Lindauer, that the programme of work carried out by the individual is not in fact primarily or exclusively determined by the physiological state of the insect, but is dictated by the needs of the colony as a whole. Moreover, it seems clear that regulation by the colony's needs must be through an elaborate series of releasers—mostly proprioceptive, though no doubt chemical releasers are also very important. Thus the individual cell-building worker seldom or never herself carries out in complete, correct and uninterrupted sequence the whole process of cell construction. Yet she does every action of this sequence innumerable times as and where required. She may, for instance, start a cell with the wax produced from her own glands, which she has manipulated. She may then put the finishing touches to another cell, using wax supplied by another bee, or perhaps a bit of light capping taken and moulded up from a cell temporarily out of use. Then again—sometimes using wax from another worker, sometimes her own—she will perform the laborious and delicate tasks of planing and polishing to produce the thin cell walls—all this in the intervals of performing duties of entirely different kinds such as feeding larvae, storing nectar or attending the queen. Much of the time she may not be nearly as active as the legend of the 'busy bee' might lead us to expect. In fact she may apparently waste a great deal of energy just wandering about the hive investigating this or that in an apparently random and useless manner, but perhaps all the while accumulating 'knowledge' of the situation in various parts of the hive and of the places where jobs of various kinds may now or later be available for the doing.[1] But whatever be the ultimate explanation of this new aspect of the behaviour, we seem quite safe in concluding that the achievement of consummatory acts in the original Lorenzian sense cannot possibly be the sole mechanism whereby the instinctive behaviour of the worker honey-bee is taken step by step along its appropriate course. At least as important, and perhaps far more important, is the perception (and organisation of perception) of the situation within the hive; perceptions which obviously are themselves goals quite as much as are any of the stereotyped actions which have been described. So here again we must speak of consummatory situations or consummatory stimuli just as much as consummatory acts. The same is probably true in lesser or equal degree of many behaviour chains which involve the construction of elaborate nests. When such a bird as the Long-tailed Tit finishes its wonderfully elaborate domed nest, properly lichened, lined, and with a perfectly constructed entrance, a great variety of different stereotyped

[1] It is noteworthy that Gleitman (1955 *J. comp. Physiol. Psychol.* **48,** 77–9) has recently established that a rat can learn certain features of a spatial pattern, without any experience of performance, but merely as a result of being passively taken along the path.

acts must have been employed, and probably a still greater variety of consummatory stimuli perceived. In this particular case at least fourteen different acts or combinations of acts have been involved, but even with all these the bird would, it seems, fail to produce a complete and perfect nest unless it had some 'conception' of what the structure should look like, and some 'conception' that the addition of moss or a piece of lichen here and there will be a step towards the ideal pattern, and that other pieces here and there would detract from it. The bird does not in fact go on building up the top of the nest indefinitely, it does not build downwards over adjacent twigs or cover them with lichen; it does not close up the entrance hole from outside—in other words, its actions are directional. It recognises when to stop, and shows evidence of having some very short-term purposiveness (not, of course, that it can possibly know, at the time of first building, what the ultimate objective of its actions must be), but purposiveness to the extent of being able to take the next minute step towards the completion of a dimly 'seen' pattern of nest structure. It is very difficult to distinguish in animals between directiveness, which is the property of all well-designed machines, and true purposiveness in the sense in which we use the word when referring to our own purposive behaviour. But it does seem to me that at present the only reasonable working hypothesis is that an extremely short-term purposiveness, a foresight of one of two tiny moves ahead, is being shown by some animals in this kind of situation;[1] and this is an important conclusion for Lloyd Morgan's position, since, as we have seen, he postulated that mental activity, at least on the first occasion in the life cycle on which a sequence of instinctive actions is performed, is percipient only. If we are right now it contains, as Sherrington also concluded, something more—a germ of perceptiveness.

The elaborate organisation of sense perceptions and the nice interlacement of learned and instinctive behaviour must be evident to anyone who thinks about the now widely familiar work of von Frisch on the transmission of information about the direction and distance of a food source from a foraging worker-bee to another worker in the hive. We now know, for instance, that the performance of a figure-of-eight dance must be dependent upon an inborn behaviour pattern of extraordinary complexity, since it is hardly conceivable that the individual worker-bee can have the opportunity to learn the relation between the general idea 'position of food source' and the orientation of the dance on the surface of the comb within the dark hive—another beautiful example of the co-ordination of inborn and learned behaviour, producing in this case a language of remarkable economy and efficiency for the purpose in hand. Some interpretation of the dance must also be inborn, but it appears that young bees are unable to forage successfully until they have first gone through a learning process and reached a certain degree of perfection of dance-following within the hive. It appears that very few young bees leave the hive on independent excursions, but nearly all get their first detailed foraging experience as a result of themselves following a dance; for although the pattern of actions of the figure-of-eight dance and its correct interpretation are both inborn, following the dance needs special practice. Inexperienced bees often find difficulty in turning sufficiently rapidly and so may appear clumsy and confused at the sharp turns of the leader. Also, since their efforts are still somewhat spasmodic, they are rather easily interrupted by the other workers crowding round, and so may have to wait until they have the luck to follow a dancer in a region of the hive where there is not too great a crowd.

Still more astonishing perhaps are the recent developments in our knowledge —again due primarily to Dr. Lindauer (1951 *Naturwissenschaften* **38,** 509–13)— as to the methods by which scout bees choose new sites for the swarm to go to. Communication is in this case also effected by dancing, which now takes place on

[1] Agar (1948 *Philos. Sci.* **15,** 179–91) indeed argues that purposeful behaviour in humans is a development of a property of all living organisms as fundamental to them as assimilation and growth.

the surface of the swarm rather than on the comb. Since the new sites sought for are always some distance away, the dance is always of the figure-of-eight type, never the round dance which refers only to near objectives. When the time comes for swarming (that is, when the hive is overcrowded with provisions and sealed brood so that the foragers can no longer process nectar), the workers are forced to abandon their normal activities. Most of them remain unemployed in the hive, but some of those who are particularly experienced become scout bees and fly out to look for a new home. These scout bees are apparently able to make an absolute estimate of the 'quality' of the sites they discover (presumably a synthesis of the properties of shelter, accessibility, ventilation, etc.) and they communicate this information as to suitability by the vivacity of their dancing. Those that dance with greater vivacity are more likely to secure an 'audience', and so there will be a tendency to concentrate interest from the beginning on those dancers which are communicating information about the best dwelling places. Scout bees which may at first advocate inferior dwelling sites may 'change their minds' if their colleagues have better homes to offer; and so, after a longer or shorter period, virtual unanimity is secured, whereupon the swarm departs to its new home. Thus the dances of the scout bees on the surface of the swarm indicate distance, direction and suitability of site. This estimate of suitability must in some obscure way be an absolute one, since it has to be appreciated in competition with the similar estimate of the other scout bees referring to other sites (Lindauer). How this complex estimation of a quality of a site can be standardised and expressed consistently as duration of 'bouts of dancing' is at present exceedingly difficult to understand.

The sun orientation mechanism of birds is now well known from G. V. T. Matthews' recent book. In its correlation between inborn faculties and experience it is remarkably similar to that of the honey-bee. In both there is appreciation of the position and movement of the sun, presumably inborn, together with an accurate internal clock. In both there is the innate ability to relate a given direction to the space-time standards innately provided, and moreover to relate the observation of these standards to the knowledge of general topography and special landmarks gained from individual experience—although bees seem unable to do the reverse and relate a route flown under overcast skies entirely by landmarks, with a particular direction. In both bird and bee there is also an appreciation of the relations between the position of a light source and the direction of movement (foraging flight, migration flight, homing flight), but the honey-bee does even better than the bird in two respects. First it must transpose between sun direction and gravity direction, and secondly it must be able to transmit information by transposition between the direction of the foraging flight and the straight run of the dance. As far as we know birds do not need to transmit such information, and so lack these additional faculties which the honey-bee possesses.

These and similar studies of orientation and perceptual organisation constitute some of the most startling recent developments of ethology. Naturalists of fifty years and more ago were deeply impressed by the complexity of the action-patterns of instincts. But they had hardly realised that innate *perceptual* organisation must be no less complex, and the sensitivity and fineness of adjustment characteristic of many organs of special sense, as now revealed by sensory physiologists and by ethologists, was quite undreamt of.

The latest example of this is the experimental demonstration by Sauer and Sauer (1955 *Rev. Suisse Zool.* **62,** 250–59) that certain warblers that migrate by night (e.g. Blackcap and Garden Warbler) can maintain orientation only if they have a clear view of the starlit sky. They seem able to do this even if they have been hand-reared and have never seen the sky previous to the experiment—the present implication being that they have an innate releasive mechanism attuned to certain features of the seasonal star pattern of the northern hemisphere.

The innate coding of such organisations in germ plasm and higher nervous centres possess even tougher problems for geneticist and neurologist than does the existence of fixed-action-patterns. With the latter there is always the possibility that proprioceptive feed-back or reafference can, on the chain reflex principle, provide at least some of the explanation, whereas in much of the former this is hardly conceivable.

Another example of the ability to perceive complex patterns and, further, to develop complex actions by elaborate learning processes by, so to speak, building upon the inborn recognition of such patterns, is provided by the studies of bird-song which we have been carrying out for some years now at the Madingley Ornithological Field Station of the Cambridge University Department of Zoology. Here the pattern perceived is a temporal pattern of sound, and the complex actions guided by and developed therefrom are the actions of singing the song. Although call notes are usually stereotyped and inborn, it has long been suspected that learning plays some part in the production of the full song of song-birds, for the imitative abilities of birds such as the Bullfinch, Starling, Mocking-bird and parrot are known to all. Nevertheless, in some species (e.g. the Corn Bunting, Reed Bunting and some species of warbler) the normal characteristic full song is entirely inborn. More interesting for present purposes are those species, e.g. Chaffinch, Linnet and Yellow Bunting, in which the basic pattern of the song is innate, but all the fine detail and much of the pitch and rhythm have to be learnt. Thus the normal Chaffinch song consists of three more or less well marked phrases ending in a final flourish. If now one takes young Chaffinches from the nest and rears them by hand individually in a sound-proof room or at least out of hearing of all Chaffinch song, there is no longer any division into separate phrases, although the song is about the right length and there is a tendency for some birds to conclude their utterance with a single simple note of higher pitch than the rest: a sort of squeak. This then seems to represent the inborn inherited basis of the Chaffinch song. If, however, after rearing such birds through babyhood, we keep two or more of them together (although still under conditions such that they never have the opportunity to hear experienced Chaffinches), a very big difference become apparent when they commence singing in earnest at about the age of nine months. Although their inborn songs are so simple, yet the attempt to sing these in company provides a mutual stimulation which encourages production of complexity. As a result of mutual imitation, each of such birds or groups of birds will build up a distinctive community pattern. Many experiments have now shown that there is a period of about six weeks' duration or less at about the eleventh month of life (when the bird is for the first time in its life in the physiological state appropriate for territorial song) and it is during this period that the full song is learnt. Once this time has passed the song is fixed for life, and it does not matter to how much wild Chaffinch song such birds are now exposed—year after year they will sing only the song which they worked out for themselves as youngsters. These and many other similar experiments make it clear what is happening in the wild. Young Chaffinches must certainly learn some details of song from their parents or other adults in the first few weeks of life. For instance, they learn at this stage that the song 'should' be divided into two or three phrases and 'should' end with a more or less elaborate flourish. But the learning of all the finer details of song is postponed by the wild bird, just as by the experimental birds, until the advent of the critical period during the following spring. This is the time when the young Chaffinch in the wild is first singing in a territory in competition with neighbouring birds of the same species, and we have good evidence that wild birds learn the finer details of the song from their immediate neighbours. There is little doubt that it is primarily in this way that local song dialects, which so many field naturalists have noted, are built up and maintained. The young Chaffinch thus learns the details of its full song by listening

to older experienced Chaffinches. How it is that it does not also imitate and acquire the songs of other species we do not yet know for certain—probably by restricting its imitative attention only to models of the right tonal quality. With the more generally imitative species there is evidence for what one must at present describe as imitation for its own sake. A Chaffinch presumably may gain some advantage in its competition for maintenance of territories by being able quickly to modify its song so as to resemble that typical of the locality, but many species more proficient as songsters and more generally imitative seem to effect what can only be described as elegant improvisation; and the biological function of this and of the more extreme forms of vocal imitation is very difficult to assess. It seems that true vocal imitation must be regarded as a late development from inherited song patterns, and must be looked upon as characteristic of the most highly evolved amongst the true song-birds. It is hard to find any selective reason for the extreme purity and elaboration of some bird notes and songs. Releaser function appears in many cases to have been transcended. This feature has suggested to several authors that in the finer details of songs we see the beginnings of true artistic creation, and there is certainly no justification for ruling out such a hypothesis as fantastic or absurd. But perhaps tonal purity, inventiveness and imitative ability are all examples of 'pre-adaptation' for apparently remote and unlikely contingencies; specialisation coming in advance of immediate adaptive requirements.

If this is true, such vocal performances are on a par with the number-sense which can be developed in many species of birds by careful training. The brilliant and painstaking work of Otto Koehler and his associates on the recognition of number by birds is one of the most remarkable developments in the study of animals' learning ability in recent years. Pitfalls in such experiments are numerous and extremely difficult to avoid, and only by the most careful experimental design and extreme pertinacity can the danger of 'Clever Hans' errors be avoided. To do this was an experimental achievement of the highest class, and as a result we now know that some birds can 'count' up to seven. Such counting ability seems to offer even less practical advantage for a wild bird than the features of song improvisation and imitation. Both are as yet set about with much mystery.

When we look at achievements such as the learning of elaborate songs and the other examples of animal behaviour we have just been considering, we realise that the distinction between appetitive behaviour and consummatory act has indeed become very tenuous. This is further illustrated by experiments on the following-response of young birds such as Moorhens and Coots. Here the very act of following a model, or the parent, has been shown (Hinde, Thorpe and Vince, 1956 *Behaviour* **9,** 214–42) to be both appetitive and consummatory. This following, which is the active maintenance of a certain spatial relation to a chosen object, turns out to be its own reward; in other words, it is the maintenance of this spatial relationship which is the reinforcement. Similarly with the imitative birds, the very act of imitating, the very fact of achieving the appetitive behaviour, seems itself to be the consummatory act.

In the foregoing examples of recent studies of animal behaviour, we see then numerous instances of more or less elaborate inborn releasive mechanisms guiding and restricting learning. We see inborn mechanisms developed by learning processes into perceptual and behavioural complexes of extraordinary elaboration, and we find, in insects and birds, instance after instance of the ability to perceive, not simple sense data but relations between perceptions both simple and complex. These faculties are in fact what have—since the coming of the gestalt school—become known as insight and insight learning. There is still, of course, plenty of room for trial-and-error; these newer results do little to diminish its importance in the story. But completely unreflective trial-and-error, what Lloyd Morgan would have called *perceptive* trial-and-error only, with no fore-plan or insight entering into

the picture at all, is probably insufficient to account for the behaviour in animals where there is any evidence of elaborate perceptual organisation. It seems to me inescapable that much modern work on animal behaviour implies that the vast majority of metazoan animals are perceptive. Moreover, in all highly organised perceptions, the perception of relations involved implies something more than this —which Lloyd Morgan would have taken to be self-consciousness. For Lloyd Morgan, the perceptive mind is conscious, but only the reflective type of mind is self-conscious; a distinction which sounds somewhat dated to our ears but which it is nevertheless worth remembering. Insight learning can be defined in Lloyd Morgan's phraseology as action as a result of insight by reflective organisation giving a new adaptive response; and a great deal of the modern evidence for insight, learning, imitation, play, number sense and reminiscence gives reasons for considering animals to be reflective in his sense. Present-day students of animal behaviour tend, not without reason, to avoid the word 'consciousness', and to perform displacement actions indicative of extreme anxiety whenever it is used with reference to animals. And we must indeed remember that perhaps Freud's most enduring claim to fame was his demonstration of the existence of highly elaborate mental processes far below the normally conscious level.[1] Yet we are justified in using consciousness as a provisional biological hypothesis whenever we find directive behaviour of such complexity that it cannot be fully or reasonably explained by mechanisms known to be operating within the body of the animal; although of course, if we can point to an efficient mechanism, that does not mean that true conscious-purpose is necessarily absent, even though, as practising scientists, we may decide that for the time being at any rate it is no longer our concern. I think further that the modern developments in the study of the higher learning faculties of animals have served to emphasise the necessity for getting back to the idea of a carefully thought out combination of objective and subjective approach, exemplified by Lloyd Morgan. The best instruments can be misused, and it is possible to cut our throats with Occam's Razor and to disintegrate our animals with a blast from Lloyd Morgan's Canon. If we try too rigidly to be objective, we may fail to see many important features of animal behaviour which become clear to us when we attempt to put ourselves in the animal's place. While of course to go to the other extreme and be too subjective in approach is fatal—we must safeguard ourselves all the time with all the knowledge we have of the animal's sensory and behavioural capabilities. But I am convinced that a proper balance of subjective and objective is necessary both for a worthy ethology and a worthy psychology. Pure objectivity on the part of an external observer is in any case hardly feasible, and if achieved will result in the neglect of much that is important. Perhaps it is excusable here to paraphrase and adapt some remarks of Lorenz (1950) without, I hope, changing their essential meaning: 'The true ethologist must be motivated not merely by a conscious effort to gain knowledge; for unless his eyes are bound to the object of his studies by that spellbound gaze, that sympathy which is evidenced by the mysterious charm that the beauty of living creatures works on some of us, he would be physically incapable of bringing himself to stare with sufficient persistence at fish, birds or mammals as is necessary to take stock of the behaviour patterns of a species.' If ethologists are right, then, that the guarded subjective approach (implying, as it does, something of the viewpoint of the artist) is one essential method in acquiring knowledge of animal nature, I believe it will ultimately be found that we have here a most important argument in support of those who conclude, as for instance does Erroll Harris (1954) (from a vast knowledge of scientific advance and scientific

[1] Freud was so lacking in the comparative approach that, except in so far as he emphasised the reality of specifically partitioned internal drive, his theorising on instinct is virtually useless to the student of animal behaviour. It seems likely that in time Jung's thought, particularly as it bears on problems of the Innate Releasive Mechanism, will be found to contain much more of value to the ethologist than does that of Freud.

philosophy), that empiricism, however necessary as a scientific technique, cannot possibly—from the nature of the case—supply an adequate scientific philosophy. But for a biologist this is treading on dangerous ground and I leave it hastily!

Finally we may ask, does this recent work on instinctive behaviour and the percipient learning abilities of animals give us any fresh view of animal nature? The answer is, I think, undoubtedly that some apparent lines of demarcation between man and animal nature have become blurred. The fact just referred to, that so many animals in certain situations give evidences of being 'reflective' in Lloyd Morgan's sense, and that, in so many cases, exploratory behaviour, the mere getting to know an environment, is itself adequate reward, at once suggests resemblances with Man. Many higher animals resemble Man in their tendency to achieve consummatory situations rather than consummatory acts. The occurrence of displacement actions in many animals when under the influence of conflicting instinctive drives shows remarkable superficial similarity to the effects of the conflict of learned or acquired drives (habits) in Man. Animals suffering from 'conflict' recall neurotic human beings in the same state to a degree that is often quite startling. Behaviour study seems then to be reinforcing physiology in stressing the basic unity of the living system in more and more of its manifestations. Some of the fundamental thought processes of animals seem far less dissimilar from ours than we once thought, and we now see that even the bees have a propositional language as distinct from the mere emotive influence which is the sole result of the language of so many animals—though of course there is no justification for assuming that the bees' communication is consciously intentional as is that of true human language. Propositional language was until recently thought to be the prerogative of humans, and indeed human language still seems to be unique in that it implies learning to use as symbols, sounds or actions which in themselves have nothing of the *quality* of the objects which they denote, e.g. number symbols.

But we may ask further, does the modern study of animal behaviour reinforce or run counter to the general trend of progress in other branches of biology? In the first place I think it true to say that it is the conclusion of many and various disciplines that the world of living things appears now as far more of a unity than was conceivable a hundred years ago. The post-Darwinian era in research has tended on the whole to break down barriers alike between phyla and between individual disciplines, and the same fundamental general principles are seen to be operating fairly widely through the animal and plant kingdom. Even the bacteria and the viruses, for all their strangeness, seem less remote than they once did. Apart from his famous Canon, Lloyd Morgan's real claim to immortality is his development of the idea of emergent evolution and the contribution his thinking made to the concept of holism. Moreover, he wrote at a time when the boundary line between living and non-living seemed as well defined as the English Channel. But how different it now is: a subtle change has manifested itself. It is not that analysis into ever finer levels of structure and physiology is showing progressively that biology is after all only the complicated physics and chemistry of an earlier generation. Far from it! To quote Dr. L. E. R. Picken (1955 *School Sci. Rev.*): 'The hope that analysis to ever finer levels of structure will ultimately reveal the nature of that relation by virtue of which the whole is more than the sum of the parts, has now been shown to be in vain. Most experimental biologists have, in the past, worked on the tacit or explicit belief that analysis in the terms of the physics and chemistry of fifty years ago —the physics of matter *en masse*, the chemistry of small molecules—would yield the answer. But we can see now that the physics and chemistry are going to be those of ordered aggregates far larger than molecules. The extent to which the concepts of chemistry and of physical chemistry have been modified sometimes almost out of existence—as in the case of the concept of the molecule—by contact with biological material is of the greatest historical importance.' Increasingly, biological organisation

now seems apparent in circumstances and conditions where not so long ago all was thought to be physics and chemistry, and it seems not improbable that future advance may come by starting with concepts derived from the biological sciences, and working back into the physical sciences. In fact the present position of biological philosophy is one which Lloyd Morgan foresaw, and some of his statements can be lifted right out of their former contexts and used in present-day writing without seeming in the least out of date.

Ethology has as yet little itself to say on this fundamental issue of living and non-living, but it does make an impressive contribution to the chorus of voices which are emphasising the essential unity of living things and the similarity, in much that is fundamental, between animals and man. But while this is true it would be a view of the utmost naïveté to assume that studies of animal behaviour imply any decrease in the stature of man. For it is sound scientific common sense to see that, if only because of the existence of science itself, man displays emergent qualities far transcending those of the organisms most nearly related to him. Moreover, his high powers of abstract reasoning, his faith, the depths of his spiritual life, his appreciation of moral values, his self-conscious discipline of the will to achieve abstract values, not only differentiate him from the animals; they and all the other manifestations which have already emerged suggest that he possesses vast further potentialities yet to be realised in both individual and evolutionary development.

THE USE AND MISUSE OF CLIMATIC RESOURCES[1]

ADDRESS BY PROFESSOR A. A. MILLER

PRESIDENT OF SECTION E

THE rain, the wind and the warmth of the sun, with all the other elements that together constitute the climate, are natural resources of this or any other country just as much as are its minerals or its soil. Yet because it has always been with us and has not, like mineral resources, been suddenly 'discovered' and subsequently 'developed', the climate has tended to be taken for granted as part of the natural order of things, regarded as a free gift to be squandered or as a curse to be endured with fortitude. It is true that intelligent farmers, through millennia of experience, have learnt how to adapt their practices to its exigencies and that water engineers from ancient Egypt to the present day have taken steps to conserve and harness one of its gifts; but until very recently little attempt has been made to review climate as a whole or to plan ways of using its benefits to the best advantage or to mitigate the punishment meted out to suffering mankind by its extremes. Floods and droughts, burst pipes and killing frosts still tend to be regarded as 'acts of God', though we have to some extent escaped from the guilt complex that led our ancestors to see in these visitations a richly deserved punishment for the wickedness of our ways, while at the same time illogically refusing to accept any blessing that might come our way in the form of a good harvest or a fine bank holiday as a signal of divine approval of our good behaviour.

But such swift strokes of disaster belong to weather rather than to climate, and weather gets its rather dismal reputation from the convention that it is news mainly when it is spectacular bad news. Jove, despite his reputation for joviality, usually made his presence felt with thunderbolts. But though thunderbolts are undoubtedly meteorology, the statistical study of the expectable frequency of the wrath of Jove is a proper study for the climatologist. In any field of planning, the first step is a survey of available resources; so in planning to take full advantage of our climatic resources the first step is fully and fairly to assess them.

It might be thought that this had already been done and that the necessary data were readily available in the meteorological records that have long been kept in all civilised countries. But while deeply grateful to generations of careful and faithful meteorological observers, professional and amateur, the climatologist is still dissatisfied. In the first place there are great spaces on the face of the earth where records are of all too short duration, and many of these are in regions of rapid and accelerating agricultural development; for example in tropical Africa, where a rapidly growing native population or the demands of industry for raw materials have rendered it increasingly necessary to replace a happy-go-lucky subsistence agriculture with more efficient and up-to-date systems of higher productivity or to substitute new crops for old. Failures have often resulted in such places from insufficient knowledge of the climate—too few observations or the wrong kind of observations. This is the basis of the second complaint. Meteorological records having been adopted in a form well tried and eminently appropriate for regions climatically quite different are not always the ones best chosen to reveal conditions in the new environment. Data that tell the meteorologist what he wants to know for his purpose and interest do not always help the climatologist to solve his very different problems. Yet the climatologist has to make do with what his colleagues

[1] Address delivered at the Sheffield Meeting of the British Association on August 31, 1956.

in the much more highly organised meteorological services provide—and he is very grateful; but we badly need more information about evaporation, percolation, run-off, river gauging, soil temperatures and microclimate, to mention only a few of the shortcomings of existing records. Meteorologists tend to be mathematically minded, while climatologists, though increasingly aware of the value of statistical analysis as a tool, tend to be geographically minded; and the pressing practical problems of planning the utilisation of climatic resources are essentially geographical. Unified only by their space relations, they are highly diversified in content and depend for their solution on a number of interdependent lines of research, each the concern of the specialist scientist; with the natural vegetation, the soil, the agriculture, the animal life, the pests, the water supply, the population, their physiology, their culture, etc. It comes to this then, that climatic planning, like any other form of planning activity, involves team work and close co-operation and co-ordination, an excellent example of which is provided by the work of the Arid Zone Research Advisory Committee of UNESCO.

In contrast with the constructive suggestions contained in their reports, the misuse to which I referred in my title is often of a near-criminal nature; there is no denying that the gifts of climate have been and still are being abused—most often through sheer ignorance. But, as with the legal code, ignorance of the laws of nature is no defence, and the punishment for breaking the law will be remitted not one jot on this plea. Thus the ignorant peasant in the Indus Valley who over-waters his fields in the mistaken belief that water is a good thing and one cannot have too much of it sins first against the irrigation authority and then against the rules of good husbandry. There may be no penalty on the first count, but on the second his sentence is severe—reduced crops, deteriorating soil structure, alkalinity and bankruptcy. Over-watering, like over-eating, is an indulgence to which those who cannot normally afford it are particularly prone—the little boy at the Christmas party, the sun-sozzled holiday-maker on the burning beach. Some rationing is necessary, not because of scarcity, but in the interest of public health and to save the addict from his own folly. As in the notorious cases of past errors of soil management in dust-bowls or the denudation of nature's defence of forest on steep hillsides in climates of heavy rainfall, the damage may be permanent. These were shocking crimes wantonly committed in ignorance, and they are now being checked by enlightenment. But there is another set of errors, errors of omission, failure to take action, neglect of opportunities, which are my principal concern in this address.

In general terms they may be included in the one word 'waste'. Our reaction to floods is to get rid of the water as soon as possible; our reaction to the drought that may follow later is to pray for rain and to bemoan all that water that we cursed and threw away. The history of man's exploitation of natural resources is a shocking story of waste; the valuable by-products that fouled the air in escaping unwanted from the early coke-ovens, the dumping of over-produced coffee in the sea, or the burning of the volatile petrols from mineral oil distillation before the internal combustion engine arrived to make a market for this dangerous commodity. These natural gifts have gone for ever, and to shed tears over the spilt milk would only be another example of waste. They were capital resources that have been squandered; by contrast our climate represents a steady annual income of heat, light, power and water; the future income will not be reduced because we have been spendthrift in the past; we shall not suffer on this account for the extravagance of our parents.

The climatic records assure us that, apart from very small differences between good and bad years and still smaller secular variations, the total annual revenue at any one place is guaranteed, though in very few parts of the earth does it come in regularly throughout the year. Rain, warmth, daylight and wind fluctuate seasonally, diurnally and, to some extent, capriciously with the weather. Too much, in the past,

we have spent each cheque as it came, surfeited ourselves with plenty, played ducks and drakes with what we could not spend, and then tightened our belts in winter or the dry season. We still have to learn how to live according to our climatic income; how to bank, for example, a daytime surplus of warmth for use during the nocturnal deficit. To design methods of equalising these maldistributions in time and in space is one of the aims of planning the use of climatic resources to which I shall return later.

Meanwhile another equally important objective of climatic planning is to take advantage of resources at present untapped. For example, there has recently been a revival of interest in the new possibilities of harnessing the wind and the sun, and important developments are impending in this field. The windmill is one of the oldest of power machines. Though clumsy as a direct drive and replaced even in Holland by the diesel pump, it is staging a 'come-back' as a generator of electricity in regions where mineral fuels are scarce or expensive. It might well be tried out, experimentally at least, at antarctic bases, where, from all accounts, wind is the one commodity they have in abundance. Think what it might eventually save in diesel oil that has to be manhandled in barrels from the *John Biscoe* to the bases of the Falkland Islands Dependencies Survey. There will be difficulties of icing, and of bracing and streamlining against destructive blizzards, but they are not unlike the problems that have been solved in airscrews on arctic-flying aircraft.

For energy use on a large scale, wind and sun both need focusing. The concentrated force of water canalised in a valley and flowing always in the same direction can be guided into the maw of huge turbines, but at present the wind-power unit is small because only the swept circle of the windmill sails or the rotor blades can be used. But hill ranges stow the wind if its direction is constantly athwart their axes, as the old iron-smelters knew when they sited their bloomeries on the bole hills of Sheffield and on exposed sites in the Forest of Dean or Coalbrookdale. The rush of air through 'windy gaps' on the ridge crests makes these the best wind-power sites on a small scale. On a larger scale, perhaps the trade winds, funnelled in valleys, also offer favourable conditions. Site studies with reference to wind constancy and speed are essential preliminaries in the early stages of the revival of wind-power.

The colossal heat energy of the sun is poured out daily over half of the earth at a time. In fact, the sun is practically the only permanent source of energy: water is raised into the clouds by sun's heat and falls as rain, to run down the rivers generating hydro-electric power; the wind blows because of differences of atmospheric pressure set up by the sun; even coal is fossil sunlight absorbed by Carboniferous plants and stored in the earth to be raped by man 250 million years later. A day of sunshine falling directly on 100 square metres, quite a small house, is equivalent to more than 500 kilowatt hours of electricity (£2 at the cheapest domestic tariff). The possibilities of wind and solar energy have been demonstrated at several international conferences held recently, and efficient means will surely be found before long of collecting, focusing, using and transmitting this energy.

But before looking into the future for new discoveries, let us see what is being done, and assess what could be done, to increase the efficiency of our present uses of climate in the oldest industry in the world, agriculture. Nourished in an atmosphere of tradition, experience and empirical lore, the agriculturist now feels the impact of science. He has learnt, with pride, that there was all the time a sound scientific basis for many of the practices that he knew collectively as good husbandry, but he is learning also, occasionally with humility, that it was not always the best possible husbandry; he has learnt that he has still much to learn.

The relation between weather and crops has long been known in general terms; now it is becoming known more precisely. The contributions of Warren Thornthwaite in this field are probably well known to all geographers, and even if some have found in his classification of climates a fruitful field for the exercise of critical

comment, they will appreciate the enormous practical value of his investigations into the growth period of fruit and vegetable crops by which he was able so to plan the planting dates for each row of beans or field of peas that each in its turn stood ripe and ready in prime condition just when the deep-freeze and canning plant at Seabrook was prepared to receive them. More recently in Uganda, H. L. Manning, analysing the rainfall expectation, has been able to advise just where, in that important Empire cotton-growing territory, the optimum quantity and the most favourable seasonal distribution of rain for raising cotton of good quality and high yield is most reliably and regularly expectable. He concludes that 'it is possible even at this stage to give regional estimates of yield of the cotton crop that should be of considerable value in the formulation of agricultural policy'. The optimum conditions and the limits of tolerance of temperature and rainfall are rapidly being determined for most commercial crops, and it is increasingly possible to find the right crop to match the existing climate anywhere. Other things being equal, this is clearly a more economical procedure than the alternative method of artificially modifying the climate to fit the requirements of the crops. However, where the existing climate is not ideally suited to crops which must, for other reasons, be grown (e.g. market-garden crops round big cities), it may be an economic proposition to provide artificially what nature withholds—hence glasshouses, cloches, irrigation schemes and hydroponics. Of course, this is not new knowledge, but what is new is the precision with which it is coming to be known how much of the deficient element of climate should be applied in order to obtain the highest possible yield and the best possible quality. The old-fashioned rule-of-thumb and feel-of-the-soil methods are coming to be regarded as anachronistic survivals into this era of the new scientific agricultural revolution.

Of all the factors exercising a strong and obvious influence on success or failure in agriculture, climate is one of the latest factors to be scientifically studied. Great progress has been made in plant breeding, pest control, fertilisers and mechanisation, but until lately it seems to have been assumed that enough was known about the influence of climate and that little could be done about it in any case. But what, for example, are the water needs of pasture land? How far are they met in the best fattening pastures of Leicestershire? The meteorological definition of 'drought' is fifteen or more consecutive days without measurable rain, and until this has happened there is no 'official drought'. This long-standing and, for its meteorological purpose, satisfactory definition can obscure the true state of affairs, and from the humble viewpoint of the grass there *is* a drought or at least a state of thirst, and though the grass does not wither, it cannot make the exuberant growth it is capable of until that thirst is quenched. No meteorological instrument has yet been devised to measure directly the water need of herbage, but measurements of evapo-transpiration from experimental plots come near it and we badly need more data of this kind. Meanwhile the water need can be calculated, rather laboriously, from existing data, and, if the weekly or fortnightly rain falls short of this optimum, we can calculate how much irrigation water to apply. Dr. Penman has shown us that in seven years out of ten, Leicestershire pastures would do much better if they were irrigated. That there is drought in England to the extent of needing irrigation comes as a shock to many people and as a surprise even to some geographers, but Dr. Penman's studies of the transpiration needs of growing grass have shown that, despite an apparently adequate rainfall, our eastern counties would benefit from some irrigation, applied as needed, every year, and even the wet West could by this means materially increase its yield, especially of early grass for cattle weary of winter stall-feeding. Among all the recourses to which we are driven in our effort to raise agricultural production in these islands there is none to compare with irrigation for achieving sheer increase of bulk. In some experiments on the University farm at Reading in 1955, irrigated plots gave from 50 to 100 per cent more

than the unirrigated control plots, and with fertiliser addition some produced more than 120 cwt. of dry matter per acre in the growing season. This was no secret to those who for generations had worked the water-meadows of Hampshire. They have been growing priceless early and late grass and multiple crops of valuable hay while growth was arrested in the unirrigated pastures above the reach of the fertilising water. Their problem has been the economic one of the cost of maintenance, and many works have fallen into disuse even though the water is free. But if we really wish to increase our agricultural production, the supply of water to our eastern counties, suffering from real but unrecognised drought, becomes an issue of national importance.

Let us now turn from water to warmth and from plants to man. Temperature has never been a serious deterrent to settlement, and man has made himself a home from the arctic to the equator. His tolerance is immense, but we shall never be able to measure the extent of his suffering, and his efficiency both of brain and of body must always have been impaired by excess of either heat or cold. Technological advances in clothing and housing now enable him to survive, even to enjoy, extremes that would have killed his naked and homeless ancestors. Indoors, at least, he is able, by central heating or air-conditioning, to create the kind of climate he likes—if he can afford it. But can we afford it? Are there not more economical ways of maintaining our comfort? Are we using the resources of our climates to the full? May I return to my earlier accusations of waste and recall that the gift of warmth is unevenly distributed spatially, seasonally and diurnally. On the grand scale I am confident that means will eventually be found of passing the surplus warmth of the tropics to the temperate zones in winter, as nature does already, not very efficiently, by means of ocean currents and the atmospheric circulation which together constitute a global central-heating system, the stove (heat source) in the tropics and the radiators (heat sinks) in high latitudes. An efficient machinery is not in sight yet, but who shall say in this technological age that anything is impossible tomorrow?—the pronouncements of yesterday are sufficient warning. Not so long ago most meteorologists would have rejected rain-making as impossible because of the colossal amount of energy involved. We can do it now (if atmospheric conditions are propitious) because we have found how to trigger off the energy that is already present. The economical procedure is to enlist the co-operation of nature and to guide her exuberant energy, not to fight her. For the implementation of these plans we look to the skill and ingenuity of the engineer in devising machinery to harness natural forces, a task in which our colleagues in Section G have seldom failed when the problem has been placed before them. As the American Engineering Corps used to say: 'If it's difficult we'll do it right away; if it's impossible it will take a little longer.'

But already within our grasp are means of storage of seasonal and diurnal surpluses against the winter cold or the chilly night. Staying last year with a Danish professor of geography, I was taken downstairs to bed. This topsy-turvy arrangement is, of course, very sensible. The mid-winter temperature of the earth six feet down at Aarhus is about 47° F., for the warmth of the summer passes down into the earth and is stored there and escapes only slowly. The bedroom walls are as warm as the earth, and little fuel is needed from the central-heating plant, conveniently placed in the basement, to keep the bedroom at a cosy temperature. Up aloft, round the bedrooms of normally-constructed houses, the average night air temperature is 5° below freezing-point, and as an Englishman must throw open his bedroom windows, the temperature inside is soon the same, though the central heaters devour the coal. In summer, even on the hottest night, the basement bedroom is as cool as a cellar—which it is. Naturally it needs attention to damp-course construction and costs something to excavate, but as a means of heat storage the possibilities of the earth beneath our feet are insufficiently realised. In fact, such is

the diminution of earth temperature range and the time-lag in downward passage of warmth and cold that less than twenty feet below the surface summer is the coolest season and winter the warmest.

This stored warmth can be transferred and stepped up by means of heat-pumps, which have been used for years in Britain and abroad, especially in the U.S.A. and Switzerland. The heat pump does not, of course, give us something for nothing, power has to be used to transport the heat and to boost it as necessary, but it gives us a lot for a little (the output is two or three times the input) and with it that satisfactory feeling that comes from using what would otherwise be wasted. Its achievement is to iron out the seasonal variations of temperature, and for that reason it gives best service in climates where there is a large annual range of air temperature and therefore a great seasonal contrast between air and ground temperatures.

Reservoirs store warmth as well as water; in fact, water bodies such as the sea, rivers and lakes, especially in hot climates, constitute an enormous storage of fairly low-temperature heat which can be used for heating, refrigeration or distillation. The Festival Hall on the South Bank is heated and cooled by heat-pump from the Thames, and at Abidjan French engineers are harnessing the temperature difference between the warm surface waters of a tropical sea and the cold waters deep down for the generation of electrical power. The heat is, of course, sun's heat, and in sunny climates this may turn out to be the best, as it is certainly the simplest method of trapping and storing solar energy. It is true that much of the sun's energy is reflected from the mirror-like surface and a great deal more is used up in evaporation of the top layers. With a view to checking this surface-evaporation loss from reservoirs, experiments have been made of spreading the surface with thin (monomolecular) films of aliphatic alcohols and other substances. Now one of the results of this treatment is that the water becomes unduly hot both because of the high absorption rate of the film and of the retention of latent heat of evaporation. It is in the hopes of remedying this that a search is going on for suitable substances which have low absorption coefficients in the long-wave end of the spectrum. But if means can be devised for extracting the heat gained and using it as a source of power, then this by-product heat becomes an asset and not a liability and the search will then begin for substances with high coefficients of absorption.

In some climates, especially arid ones, the great diurnal temperature range brings alternating forms of discomfort, heat and cold. Over such a short period a simple heat trap can meet the case. The sun's rays are made to pass through a glass lid into a big black box sited on a sunny roof or wall, and the light waves, transformed into heat, are absorbed by the black lining; the air thus heated in the box is then passed into a storage bin filled with gravel. At night the cold air is passed through the box of hot gravel and goes on to warm the house. There are other ways of equalising daily extremes, e.g. crystals of sodium sulphate, which melt by day (at 90° F.), absorbing latent heat of fusion, and recrystallise by night, releasing the heat (about 100 B.Th.U. per lb.); more efficient devices are being developed in countries where the methods are appropriate. The traditional architectural design of desert regions is well adapted to its climate. The thick walls virtually constitute a heat trap, and the small windows cut to a minimum the entry of heat by day and the loss of warmth by night. But immigrants, in their ignorance, have sometimes been slow to learn the lesson. Generations of Australian settlers in the out-back or in the mining towns suffered unnecessarily, sweltering in houses or shacks of corrugated iron, a material which, though cheap and rainproof, has heat-transmitting and radiating disadvantages which are about as bad as they could be.

The insulation of buildings against heat loss is a field of research which becomes more urgent every time the price of coal goes up, and the Building Research Station is fully alive to it. So is the Fuel Research Station, so are the architects, so are the climatologists, and so must we be as geographers, since housing design is an

expression of adaptation to the climatic environment, whether it be unconscious and traditional (based on experience) or deliberate innovation (the application of new discoveries and technologies).

I hope I shall not be told that this subject of heat conservation 'is not geography' because it is too specialised. Admittedly it is only a part of a part of climatology, and climate is only one of the geographical influences at work in shaping the ways and works of man, but it touches him in so many sensitive spots and influences such a variety of his activities that specialists of many kinds must be concerned each with some limited aspect. Each has a clear and penetrating view from his own angle, but blinkered, inevitably, by the narrowness of his angle of vision. Now I maintain that what the geographer has is not a 'point of view'; his is, or should be, the all-round and all-over view, a synthesis of all points of view. May I illustrate this by one last instance—water. There is a whole complex of problems related to fresh water and man's need for and use of it.

Of all the elements of climate rainfall is perhaps the most unfairly distributed over the face of the globe, and man's attitude towards water varies with its abundance. Where water is liberally given it is not valued, where it is withheld it is the most precious thing on earth. A prospector dying of thirst in the desert would give all the gold in his pockets for a little of the priceless stuff that runs with unmetered lavishness through the bath-tap at home.

In arid climates then, man is a water-worshipper and must be a water-hoarder. Not for him is the extravagant consumption of water for personal or industrial use, every drop that he does not need to drink is wanted for his crops. Irrigation is his salvation, and it is a fact, no less remarkable because it is so well known, that water shortage was the spur of adversity that first drove arid man towards civilisation in the Nile valley and Mesopotamia, while man in the more humid climates of Western Europe wallowed like a hippopotamus in plenty of water. Foresight and planning were the secret of their progress, and, though we are only just beginning to realise it, planning is soon going to be just as necessary for us hippopotamuses.

In their case the spur was seasonal scarcity, or, in the modern jargon, peak output was out of phase with peak demand; the demand for water did not coincide with maximum supply. In passing it may be noted that supply and demand coincide in continental climates, where rain comes at the hottest period of the year, but are opposed in maritime climates, where the maximum rainfall tends to be in winter, but maximum evaporation losses and the greatest demands of growing crops are features of the summer season.

In the main ours is a maritime climate, but the summer scarcity of moisture, though fully appreciated, is not apparent from the crude rainfall figures. At some stations, in fact, the midsummer months actually have the heaviest rainfall. We turn to the temperature data for an explanation of the paradox; the extent of evaporation at summer temperatures is not always as fully appreciated as it might be. Our islands are generally ranked in all classifications of climates as 'humid with no dry season', and it is generally held that we have plenty of water; but its distribution is certainly not the best that could be designed and its reliability in place and in time leaves much to be desired.

Despite summer evaporation, our generous yearly rainfall would be more than enough if we could use it all; as a matter of simple arithmetic the average rainfall of England and Wales would provide about 31 cubic miles of water a year, enough for 2,000 gallons a day for each man, woman and child of the population. At present, about half of this passes, most of it inevitably, back into the air by evaporation. Most of the remainder runs away to the sea in the rivers, though admittedly much of it has run usefully through our fields, our reservoirs, our sewage works, paper mills and round the furnaces, cooling towers and vats of industry on its way. How much of this can we save to use and how can we save it? The survey of resources

has already been made, abundant data have been carefully collected and assessed by the Meteorological Office and the Geological Survey (for underground resources). Consumption figures are known too, and it is no secret that when the two are placed side by side there is reason for grave concern. What is most alarming is the rise in consumption, especially by modern industry. By the latter half of the nineteenth century the amount of water required for industrial purposes in Lancashire and Yorkshire was equivalent to 8–10 inches of rainfall on the gathering-grounds of the Pennine Watershed. A bleaching, dyeing and printing works may use for process work 1–2 million gallons per day, the production of a ton of steel needs 65,000 gallons of water, the preparation of 1 ton of sulphite paper uses and fouls 64,000 gallons. Such figures may look quite small compared with the demands for cooling atomic power stations, for which an adequate water supply must be a prime locating factor.

Domestic consumption, too, is going up as more people get unmetered piped supplies. A bucket per person per day could suffice when it had to be carried, but in America, the most plumbing-conscious nation in the world, the consumption in the great cities has reached 133 gallons per day per person; cleanliness is clearly outstripping godliness. Holland, on the other hand, only used 30 gallons per day per head, for water is scarce and its use is carefully controlled. Liberal use of water is a good thing, and, with our rainfall, it should be every Englishman's birthright; but there is clearly extravagance.

With the expansion of our cities and the mounting needs of industry, no wonder the public water-supplying corporations are alarmed; but there is worse to come. Many private concerns and industries extract water from rivers and wells, and the combined effect of their pumping is drawing so heavily on our underground resources that there is real danger of exhaustion, not to mention the contamination of aquifers when the level of the water-table is so lowered that brackish or saline waters seep in. In our text-books the London Basin is given as an example of overflowing artesian wells; it is so no longer, and has not been for many years. The water-level in London's great underground gault-lined chalk reservoir is falling at the rate of up to 15 feet per year, and, though to a less extent, the same fate is threatening many of our other aquifers. A veto has been placed on the sinking of more wells, save with the permission of the Minister of Housing and Local Government, but the damage is done and the accumulation of hundreds of years of rainfall is being squandered. Normal rainfall on the chalk outcrop cannot hope to replenish it. Can the water be put back? This is a question to which 'no' is unacceptable as an answer. It is a problem for which a solution must be found. In Sweden they recharge underground supplies by allowing water to run into basins like bottomless filter-beds and soak into the underlying porous esker sands. Germany uses her fluvio-glacial gravels and Holland her dune sands for underground storage on a grand scale. It is true that the chalk is not so pervious as these superficial deposits, but certain areas are receptive where minor flexures have opened up the joint system. Through these, or by some other inlet, I am confident that geologists and water-engineers in co-operation will find a way to divert surplus floodwater from the Thames and its tributaries, or water brought from elsewhere, into that natural reservoir that is geology's gift to the metropolis.

The main locating factor for London was not in the first instance its water supply, though many of the villages around were located by wells, but it is vital for its continuance; for in respect of surface supplies London is at a great disadvantage compared with our other great cities, being situated in the driest quarter of England and, unlike lucky Sheffield, having no mountain valleys for reservoirs near it. Surface storage in the London area is too extravagant to be contemplated except in the very last resort. The relief is so low that reservoirs must be quite the wrong shape, large and shallow, holding little and losing prodigiously from summer evaporation,

for the Lower Thames Basin has the highest summer temperatures in Britain and, closely related, the highest evaporation rate. The land they would have to occupy is valuable for agriculture, industry or residence and cannot be spared. This concatenation of incorrigible circumstances can be countered only by imaginative planning and engineering genius, exercised along two lines. The first battle to be won is the replenishment of the underground reservoir, where the water will be exempt from evaporation losses. And it must be made rechargeable at a rate that will not only keep pace with the present extraction rate, but also provide for the increase that our experience shows to be inevitable if London is to be allowed to grow still bigger and more industrialised.

The second battle is to get the water to London by surface flow or piped supply —somehow. The Thames is not enough, and in any case it must be remembered that the Thames water that comes to Teddington has already been drunk, bathed in or passed through the laundries of Oxford, Reading, Maidenhead, Slough and Windsor, to mention only a few of the communities on its banks that want to use its water in increasing volume. To look at the average discharge at Teddington Weir (about 1,350 million gallons per day) one might think this was enough for ten million people even on American standards of domestic consumption, but industrial and other users expand the consumption enormously and unpredictably. And in any case the average discharge includes unusable floods (rising to 11,000 million gallons per day in the floods of 1894). Though the engineers work miracles, a large proportion of this must escape to the sea, generally through the houses unwisely built on places clearly marked on the Ordnance Survey maps as 'liable to floods'. Sooner or later, water will have to be brought to South-eastern England. It is not just a matter of supplying London, insatiable thirsty monster that it is. There are other towns in the lower Thames Basin, some of them crying out for water in summer. And, as I pointed out earlier, agriculture and horticulture need great quantities for supplementary irrigation if they are to respond to the demand for increased production of milk and vegetables.

The outstanding feature of our water problem is the imbalance of supply and demand. The heavy rainfall is in the West; the heavy demand is shifting from the hill-foot industrial regions of the coalfields and is increasing in the South Midlands and the East, as new industries develop there, as population migrates that way, and as the irrigation need is recognised. The hilly West traps the water from the rain-bearing westerly winds and spills it in three-figure rainfalls on the mountains of Scotland, the Lake District and Wales. Some gets through, but the Lower Thames Basin has less than twenty-five inches a year. This was enough by the standards of last century, but it will not do for this. Something active must be done to equalise this spatial inequality. The West has more than it wants, land-drainage and water disposal are more urgent needs; the East has not enough, it needs and will increasingly demand water for domestic, industrial and irrigation purposes. Providentially, where the rainfall is high, the rocks, being impervious and hard, are suitable for reservoir construction, and where the rainfall is low the rocks are porous and suitable for underground storage. The general gradient is downhill, though the distances are great. The engineering problems are prodigious, but the urgency of the need insists that they be overcome. It is nearly 2,000 years since Rome was supplied by nine gravity-fed aqueducts; today 600 million gallons a day are brought 126 miles from the Catskills to New York and Coolgardie is supplied by a pipe-line 350 miles long. These are challenges that we can meet and beat.

In previous crises in our history we have surmounted the difficulties of regional inequality, and the canals and railways and pylons in our landscape are monuments to these achievements, as the aqueducts, like the Pont du Garde, are to the skill of the Romans. The railway system helped to even out the maldistribution of our coal supplies, and the coal trains ran with large loads from the mines of Glamorgan to

the grates and factories of London. Water, we are just beginning to realise, is a commodity as valuable though not yet as costly as coal, and is much more transportable. You can buy a ton of water transported from Rhyader delivered at the tap in Birmingham for about threepence, but a ton of coal costs a little over £3 to raise, and the price has trebled by the time it is delivered to the coal-shed. Again millions of pounds are being spent on our roads, which we recognise to be the arteries bringing life-blood to the new light industries. Can we not spare a little energy and expense to make the precious water flow from our water-laden West to fertilise our eastern fields and factories.

Actually, for irrigation purposes water does not need to be pure and potable. The expense of filtering and perhaps chemical treatment would seem to be an extravagance for water that is destined to be scattered on grass that may not like the chemicals in any case. The supply of suitable low-grade water for such purposes is a matter that might repay fuller consideration and some undertakings have already given a promising lead. Croydon uses sewage effluent for the cooling towers of its electricity power station, and there are praiseworthy examples of paper mills making use of hot waste water from chemical works wisely located nearby—a form of industrial symbiosis much to be encouraged. Unfortunately, such good-neighbour policy is the exception and users are more often in legal conflict than in such malodorous harmony. The interests concerned are legion: fisheries and canal transport, recreation, sewage disposal, agriculture, hydro-electricity and industry (private and national), as well as urban and rural water supply. Often the interests are conflicting; the reconciliation may have to be a compromise, but it must be effected in the interests of the nation. In its uncoordinated expansion from the village well or parish pump to more than a thousand district councils and water companies, the water-supply question is in danger of becoming a competitive scramble for water to drink; and in reconciling the numerous other interests in the river or on its banks the river boards are faced with complex problems, that are met but not often solved by litigation. The time has come—indeed, it came some time ago—for planning and action on a national scale and, in the first place, for the exercise of those powers that were conferred on the Minister of Housing and Local Government in the preamble to the Water Act of 1945.

In the enthusiasm for planning that has swept this and many other countries in recent years, the planning of our water resources has been curiously overlooked, or deliberately thrust from our minds. It was a geographer who drew attention to the need for a Land-Use Survey, the value of which has been amply demonstrated over and over again; there is a still stronger case, I suggest, for a Water-Use Survey. The facts of water use are known, as indeed were the facts of land use, but they have not yet been brought together in a form in which their inter-relation can be readily appreciated. The distinctive feature of the Land Utilisation Survey was, in essence, the use of the geographer's tool—the map. It would not be difficult to devise a cartographic notation that would display synoptically the quantity and quality of water that is used for each of the varied purposes to which it is now being put. Placed side by side with another map showing what is available from rain, rivers and underground sources and second-grade effluents of various kinds, it would reveal the situation in its strength or weakness with a clarity needing only brief accompanying memoirs for the completion of the picture as it is today. The revelation of areas of surplus and areas of deficiency and the distribution and quality of effluents could form the starting-point for planning the future.

A water-use survey such as I am advocating is only a part, though probably the most urgent and rewarding part, of a general survey of our national resources of climate, leading to a fuller realisation of their potentialities for agriculture, housing health and happiness. It is a pleasure to pay a tribute to the government research departments that are assiduously collecting data and producing maps and atlases

of various individual distributions within their specialised spheres. Though they would be the last to claim that they have all the answers to our problems, they are very knowledgeable and very wise, though very cautious. One thing that is needed is the fuller education of the public in making use of the advice and information services provided, at the public's own expense, by, for example, the Air Ministry and the Geological Survey, whose respective spheres meet and mingle on the geographer's own field of study—the surface of the earth which is the home of man, the plane on which he permanently dwells.

The specialist sciences take nature to pieces to find out how each part works; geography reassembles the pieces and shows how they cog in together. My dictionary defines 'concrete' in two ways: (1) the opposite of abstract, and (2) a mass formed by parts sticking or growing together. In both these senses, here, in the co-ordination of these discrete problems lies the geographer's opportunity to make a concrete contribution. So may I, in conclusion, point some of the implications for the geographer inherent in what I have said or hinted in this address.

The location of settlement and the location of industry have from the beginning been closely tied to the availability of water. Early settlement, whether spring-line, well or river-bank, sought out naturally-occurring supplies; today, settlement follows the mains in ribbon development, where water can be had cheaply and easily, at public expense. We have come to regard water as a commodity with which the householder has a right to be supplied anywhere. Settlement, in the form of new towns, garden cities, suburbs or industrial villages, is now planned in advance and such is the faith in the universal availability of water that the settlement site, selected for some other reason, now locates the water supply, reversing the old-fashioned nexus of cause and effect. Such settlement must of necessity be concentrated, nucleated or linear; in developed countries dispersed settlement is a thing of the past; it happens today only along the pioneer fringe where settlement history is still being written on the landscape.

Early industry, textile, chemical and metallurgical, drew its water from rivers or its own wells. Now, thanks to mains supplies, it is virtually liberated from these geographical controls; a water grid would give to industry a final degree of 'footloosenesss comparable with and added to that achieved as a result of the supply of power from the electricity grid.

And what will be the results of harnessing the power of the future, power from the sun, wind and the heat of earth and water, when efficient means are found of harnessing them? A whole new map of power will bring a new set of interesting possibilities; and technological man, more than ever master of the possibilities, shall be the judge of their use. The region of occurrence will, of course, benefit from the production and sale of the commodity (if commodity is the word), but factors other than the location of occurrence will govern the geographical distribution of its use. For of all the achievements of the present century, perhaps the most significant has been the revolution in transportability, by pipe-line or cable, of oil, water and electrical power; and the revolution does not yet appear to have lost its momentum.

Perhaps this seems remote—something far away in the future. But I do believe it is near and needs thinking on now. For the growth of technological knowledge is an accelerating chain-reaction, gathering speed from what it discovers and achieves. In climate, as in atomic physics, we are on the brink of realising new sources of energy, discovering new uses for old forgotten and neglected gifts of bountiful nature. Where they occur and where they will be used must be matters of great concern to the geographers even of this generation. How they will be used is a matter for technologists, and the ends to which they will be put will be a grave responsibility for the statesmen of the future. Man's perverse ingenuity in turning good to evil ends makes one chary of forecasting the future, but in the developments that I have hinted at I can only foresee their use for good.

PRICES — AND COMPETITION — IN TRANSPORT AS AN INSTRUMENT OF POLICY[1]

ADDRESS BY PROF. GILBERT WALKER

PRESIDENT OF SECTION F

I

THE economist, trained in the classical tradition as indeed I was, is bound to respect the prices formed in a competitive market as an institution for distributing resources between the several employments in which they can be occupied. The argument is well known. The charging of a price effectually discourages all those the urgency of whose demand for the goods or services in question is less than their disinclination to pay the price. Supplies are confined to satisfying wants of the greatest urgency; and the more urgent demands, measured by the purchaser's willingness to pay, are satisfied before scarce supplies are absorbed in the satisfaction of demands the urgency of which is less. If the urgency of a demand to the individual is any indication of its importance to the community —and to all of us committed to the principle of consumers' choice it cannot be otherwise—distribution by price ensures that scarce supplies go where they are most needed.[2] Demand, to paraphrase Alfred Marshall's famous simile, is one only blade of the scissors. The other is furnished by the cost of production.[3] Prices in a competitive market cannot fall permanently below the cost of production. No supplier, deliberately and for long can afford, nor indeed, should he be expected, to work for less than the addition to outlays already incurred including an element for the normal profits of the trade. Competition between suppliers holds prices to the costs at which the particular services can be provided by those already in the trade; and price, where there is free entry for capital and enterprise cannot rise more than temporarily above the average cost of production except by an amount which depends upon the customers' preference for the one carrier, the absence of any real alternative or ignorance that there is any!

There is, of course, no free access to the transport market. Competition as I have remarked elsewhere[4] though active is neither perfect nor pure. Nobody but the British Transport Commission may start a new public line of railway and entry to the trade of road haulage lies in the discretion of the licensing authorities. Only the private carrier is free to run more lorries as he pleases and he, by the terms of his licence, may not carry for others. Transport, as one member of the Commission has told us, is not a commodity which, once produced, can be stored and withheld from sale until there is a prospect of a better market. The conveyance of a consignment or a passenger is a service which has to be consumed at the moment of its creation or be lost for ever. A capacity for passengers around the middle of the day, consisting of empty seats on the 12.40 p.m., entirely disappears when the journey is ended. The railway wagon or the carrier's lorry would not be run unless receipts from the consignments it carries at least cover the outlays directly incurred in the journey.

[1] I have had the advantage, when preparing this paper, of discussions with Dr. Michael Beesley and Mr. Alan Walters, both of the Faculty of Commerce and Social Science in the University of Birmingham.

[2] The parking of cars (and of other vehicles) on the public highway is at once topical and an excellent illustration of the point. A piece is appended.

[3] Marshall, *Principles of Economics*, 8th Edition, Book V, Chapter III, Section 7.

[4] See my *Road and Rail* (2nd edition), 1947, p. 26.

The cost of transport includes all the expenditures laid out in providing for traders with traffic to consign and fare-payers wanting to travel. But the prices compared by the buyers are the rates for the consignment and the fare for journey. The latter do not always measure real cost as the amount of resources consumed by the several purveyors of transport in performing the services the subject of the competition; and the former are open to be undercut by operators not responsible to the public for the regularity of the service. The construction of the lorry and of the bus has to satisfy certain statutory rules and orders. The operations of the carrier must conform with statutory provision and regulation governing speed limits, wages and the hours for which drivers may work. But carriers by road undertake no such legal obligation as the liability imposed on the British Transport Commission to find each year, the interest guaranteed by the Treasury upon the stock issued in compensation to railway proprietors. Motor operators do not pay for the use of the road over which their vehicles run but they are taxed both on the vehicle itself (registration duty) and at 2*s.* 6*d.* on every gallon of fuel they burn. In common with all private business, professional motor carriers have to make both ends meet (and pay their taxes) or go out of business by bankruptcy or otherwise.[1]

Mechanical inland transport on the surface (I know that the omission will be forgiven by any inland waterways men who may be among my audience) is supplied today by British Railways and by road motor carriers both privately and publicly owned. The first are controlled by the law and by their own sense of the obligation owed by a great undertaking to a public who, since January 1, 1948, have also been their owners. The second, excluding the traveller in (and on) the $3\frac{1}{2}$ million private cars and 1 million motor cycles, are governed by a number of acts of the British Parliament and by those parts of the Transport Act of 1947 as amended by the Transport Acts of 1953 and 1956, which regulate the activities of the British Transport Commission as carriers of goods and passengers by road. The Transport Act of 1953 has removed the most onerous of the disabilities previously bearing upon the railways. The business of carrying goods by road motor for hire has been reopened to the private owner. For many years, as some of my audience will know, I have been an unregenerate upholder of free competition in transport. That principle has now been adopted in the Transport Act of 1953, and the occasion is appropriate to examine some of its consequences.

The Transport Tribunal, on the application of the British Transport Commission to confirm a schedule of maximum rates under Clause 20, Section 2, subsection (*b*), of the Transport Act of 1953, sat for forty-three days in 1955 and 1956. Evidence and argument occupy 947 closely printed folio pages.[2] The Commission propose to base their scales of maximum rates upon the loadability of the merchandise, upon the weight, that is, which can be loaded into a normal 10-ton truck. These maxima are assessed as the sum of the costs to which the railways are likely to be put in carrying a consignment of given weight in adverse circumstances. 5–10 per cent of all traffic by volume, it was estimated, might be carried at these maximum rates. The cost of such traffic was expected, on the average, to

[1] The annual proceeds of tax raised upon motor vehicles and upon motor fuel now exceeds by four or five times the sum spent annually upon all highways by all authorities. The proprietors of buses and lorries taken as a class are paying collectively in tax more than the annual expenditure upon the roads which they may be supposed to use. I know of no data drawn from British experience bearing on this point, but I assume that the taxes raised specifically from owners of these heavy vehicles are sufficient at least to recoup any costs to which the highway authorities are put in furnishing the track over which these vehicles are run. See the presumed distribution of taxation raised from the several classes of motor owner prepared by the British Road Federation and reproduced in the current issue of the *Journal of Industrial Economics*, June 1956 (Walker—'Highway Finance').

[2] In the Court of the Transport Tribunal: Transport Acts, 1947 and 1953: In the matter of the application of the British Transport Commission (1955, No. 2): To Confirm the British Transport Commission (Railway Merchandise) Charges Scheme. These proceedings, and especially the evidence of the witnesses for the Commission, are warmly recommended to students of economic policy.

be twice as much. This excess of cost over rate the Commission are willing to absorb. The Commission, following the practice of many another commercial concern, can be expected to issue price lists. In addition, there will be the " great variety of charges " negotiated directly with the trader, at the discretion of the Commission with neither condition nor limitation. The obligation to publish the rates paid for freight has now been repealed. Nothing need be disclosed except the legal maximum, unless the trader authorises the Tribunal to remedy complaints from traders who allege, successfully, that the railway rate is unreasonable and that their traffic could not reasonably be carried by any other means.[1]

The Commission are obliged by the Act of 1953 to dispossess themselves of the business of carrying goods by road and they may not now purchase road passenger services except by consent of the Minister. The Act also empowers the Minister to require the Commission to dispose of their present holdings in bus and coach companies if he sees fit.[2] Amended by the Transport (Disposal of Road Haulage Property) Bill, 1956, the law now permits the British Transport Commission to retain the lorries required to maintain the trunk network built up by the British Road Services. All services for hire and reward, whether worked by publicly owned undertakings or by private operators, are once more prohibited except under licence.

I add a statement showing the total estimated expenditure upon transport in all its forms. The grand total of the national expenditure at factor cost (excluding, that is, indirect taxation but including subsidies) in Great Britain in 1953 is estimated to have been £14,540 million. Of that total, current expenditure upon inland transport was at the rate of 15 per cent.

Gross Expenditure in Great Britain on Road and Rail at Factor Cost and Net of Duplication in 1953

£000,000

	Freight	Per cent	Passenger	Per cent	Total	Per cent
1. Transport Industries:						
Rail . . .	299(*a*)	26	130(*b*)	13	429	20
Road . . .	176	15	273(*d*)	28	449	21
Total . .	475(*c*)	41	403	41	878	41
2. Private Transport:						
Road . . .	676(*e*)	59	579(*f*)	59	1,255	59
GRAND TOTAL .	1,151	100	982	100	2,133	100

I am indebted to Mr. A. A. Walters, Lecturer in Statistics, Faculty of Commerce and Social Science in the University of Birmingham, for permission to reproduce this table.

(*a*) Including collection and delivery by road. Excludes miscellaneous receipts such as demurrage, fees for customs clearance certificates, lavatory takings, etc. Source: *B.T.C. Report and Accounts*, 1953.

(*b*) Including London Transport (Rail). Excludes shipping services such as cross-Channel passenger ships, etc. Source: *B.T.C. Report and Accounts*, 1953.

(*c*) B.R.S. gross revenue from carrying operations *plus* an estimate of the expenditure on carriage of goods by 'A' licence, 'A' contract and 60 per cent of 'B' licence vehicles. Source: *B.T.C. Report and Accounts*, 1953.

(*d*) Includes taxi-cabs, tramways and trolley-buses.

(*e*) 'C' licence, 40 per cent of 'B' licence operation and goods-carrying vehicles operated by Government Departments including Post Office.

(*f*) Excludes pedal cycles and hackneys.

[1] Transport Act, 1953, Clause 20, Section 3 (*d*); Clause 21, Section 1 (*d*); Clause 22.

[2] This power has not been exercised and I doubt if it will be.

The Commission have not concealed the principles on which they propose to make their charges, nor have they made any attempt to do so. The price of rail transport in the future, as the Chief Commercial Officer of the British Railways has argued, must reflect—

> a very great variety of operating conditions such as . . . the weight and loading characteristics of the consignment, the volume and regularity of the traffic and the volume and balance of the traffic over particular routes. All these things are just as familiar to us; but they are not, at the present time, reflected in the structure of our rates system in that kind of way. If the railways are to be competitive within their proper sphere and are at the same time to fulfil their duty to pay their way, it seems to me that their charging methods must on the one hand be such as to display a wide measure of elasticity in application, and on the other, they must conform to a general pattern which pays a good deal more regard to the particular character of the operations entailed and the circumstances in which they are performed.[1]

Some averaging of costs between one freight and another of course there must be, by the Commission as by any other large concern, as witnesses for the railway were quick to point out. But there is room no longer for that wholesale cross-subsidisation [2] of one traffic by another which was (and is) a usual feature of the conventional system of railway rates based on the uniform mileage scale, the classification of goods by value and the prohibition of undue preference. All these elements of equity between traders requiring transport in unlike circumstances will be gone from the rates for railway transport now emerging from the Act of 1953. Should the direct cost by rail be demonstrably higher than the charge at which the traffic could be carried by road, the railway, so we are now informed, may refuse to compete, either by charging a high figure, possibly as much as the maximum or by withdrawing the service entirely. Freight costly to convey is to be found wherever there is irregularity in dispatch, awkwardness of loading and little to bring back, even supposing the merchandise to be consigned over a route otherwise well patronised. Nobody can properly object, though many might be tempted to do so, should he be asked to pay high for freight when direct cost by rail can be shown to be high and there is road carriage to be had at the lower price. But what of those consignments which do not attract the road haulier and are sent to places from which there is no weight of traffic back? There must be whole areas—the rural districts and the remoter regions generally—accepting and receiving freight so light that no regular service could be expected to pay.

The Herbert Committee, reporting on another nationalised industry, were led to remark that 'matters of supposed national interest, if they conflict with the economic operation of the industry, should be recognised as essentially political matters to be decided by the Minister subject to his responsibility to Parliament'. Earlier they had said that 'it is not for the persons running the industry to undertake uneconomic schemes of development within rural or urban areas in the supposed national interest, if the effect is to subsidise one particular body of consumers out of the pockets of others'.[3]

The privately owned public carrier by road cannot be asked to shoulder an obligation to carry at rates which do not pay nor to places which cannot provide a profitable traffic. Before the Act of 1953 the railways were under an

[1] Question 524. Some of the consequences of permitting competition between railways, compelled by law to charge according to statutory (and conventional) principles, and road hauliers, free to make what rates they pleased, have been extensively canvassed by the Commission before the Tribunal, in their annual reports and in a paper which I read before this Section when we met in Leicester in 1933. See my contribution in the *Economic Journal* in June of that year.

[2] Cross-subsidisation is an apt term popularised by the British Transport Commission to denote that transfer from the more profitable to the less which occurs when services which cost different amounts to provide are charged at the same price.

[3] Committee of Enquiry into Electricity Supply Industry: (Herbert) Report, Cmd. 9672, para. 373.

obligation to carry at the appropriate standard and exceptional rate.[1] Now the Commission can point to their statutory undertaking to levy such rates, fares, tolls, dues and other charges 'as to ensure that the revenues of the Commission are not less than sufficient for making provision for the meeting of charges properly chargeable to revenue, taking one year with another',[2] and can plead their inability to go on affording facilities for transport except at rates which cover at least the direct costs incurred.

II

The British Transport Commission proposed in 1954 to spend on their railways over the next fifteen years, some £1,200 million, a sum just about equal to the figure at which the properties were taken over on nationalisation in 1948. Beesley and Walters [3] have estimated at about 9 per cent the gain the Commission hoped to make; and about half the money is to be raised in the market, where the requirements of the Commission (supported by the Treasury guarantee) will compete with private demands. What is the present cost of the road improvements announced by the Minister to the House in February 1955 is not precisely known (even if the programme were). Experts more knowledgeable than I have attempted their estimates of the probable cost in the large of the congestion and of the delays to which traffic is subjected on the Queen's highway. They have arrived at the happy conclusion that the money sunk in a motorway or other considerable improvement by public authorities would be well repaid in the savings enjoyed by the users in time and in the reduced expenses of running their vehicles. It should always be possible, if I may borrow a phrase from the White Paper on Employment Policy of 1944, for a 'small central staff' to compare the financial returns expected from the investments projected in railways and in roads—and in waterways and airways for that matter. There is unhappily no 'official organisation with the knowledge and reliable information' [4] required for the task. Railways are the responsibility of the British Transport Commission. Investment in trunk roads is financed by the Exchequer, administered by the Ministry of Transport and undertaken by the 1,286 local highway authorities who do the actual work. Scrutiny of this expenditure, we were told in evidence before a Select Committee of the House of Commons in 1953, has been designed to facilitate oversight by Parliament of public moneys spent by the Ministry and of grants from the Ministry spent by the officers of local authorities. But at no point in this double review is the opportunity taken to consider the improvement of communications by road as a question of policy and the relation of an expenditure upon highways to an investment in the railway. The House of Commons debated plans for the modernisation of railways just six days after they had listened to the Minister announce the expanded road programme. No wonder the member for West Bolton was moved to ask 'how the Government proposed to decide whether, financially, its size (the Commissions' plan for railway

[1] The obligation to charge no more for transport by rail than a certain sum fixed by law, in spite of the considerable variation in the costs of working has been eased in the past by the closing of a station, the withdrawal of the more expensive passenger service or the abandoning of the line outright. Much of the time of the Consultative Committee has been taken up with these discussions. Such schemes have reached Parliament and, on a recent occasion, drawn a statement from the Minister (537 H.C. 1672). The Commission can and do support their case by figures demonstrating the loss to which they are condemned by the continued provision of the facility and the opposition can be non-suited, morally at least, when the Commission show, as they so often can, how few of the objectors have recently used the station or the line! Walker and Maddick 'Responsibility for Transport', *Political Quarterly*, 1952; *British Transport Commission Report*, 1955, paragraph 30.

[2] Transport Act, 1947, Clause 3, Section 4. The obligation was not abrogated by the Transport Act, 1953.

[3] *Westminster Bank Review*, May 1955.

[4] Cmd. 6527, para. 81; Cmd. 7046, 1947, para. 12 (*a*).

modernisation) was right or whether it is much too big or not nearly big enough.' Had the Minister, Mr. Arthur Holt went on to say—

> been talking to a group of wealthy people who were to be asked to put money into the scheme he (the Minister) would not have convinced us that we should part with our money and give it to the railways. He did not convince us that we would get a good return or even some return or a better return than if we put our money into, say, electricity or roads.[1]

We were encouraged during the 1930s to see in capital outlay the key to an increase in employment and in times of general distress to measure the advantage of investment by the addition which resources used for some non-current purpose could make to the men and machines brought into gainful occupation. Close on half the year's total gross domestic capital formation is today in public hands, including therein both public departments and nationalised industries. There is no question now, nor can there have been since the War, of applying the national resources to non-current purposes in order to make work. When, in 1956, a scheme of public investment running into millions of pounds a year is decided upon, it can be only because, in the opinion of a body of officials, the condition of the country demands that resources be invested in this particular improvement—or that the central financial authorities are once more reassured about the risks of inflation.

Officials of the Ministry of Transport were satisfied, we were told in 1952, if outlay on highways was kept within 60 or 70 per cent of the total expenditure devoted to that purpose before the War. The £80 million or so being spent on the roads in 1952 amounted to £32 million in 1938 values, and the real resources applied to road maintenance and repair had thus fallen by half since 1938.[2] A much greater expenditure on roads is now contemplated rising to some hundreds of millions a year if the interested parties get their way. The authorities who lay out funds on the highways are not also the carriers. The costs of an investment in roads are borne by the public. The gains are enjoyed by the users, including the millions of private motorists. In assessing the return to an outlay on highways, there is to be reckoned in addition to the lessened expenditures of those whose costs can be counted, the greater satisfactions of motor owners generally. Most of these cannot be valued in pounds, shillings and pence and many of the beneficiaries being private motorists may not be susceptible to the financial calculus.

III

The charges of the public carrier cannot for long exceed the figure at which the trader might do the job for himself, running his own lorries for the purpose. This cost owing to the legal restriction which prevents a trader from carrying in his own vans none but his own goods, may rise to a figure representing a haul loaded one way and empty in the other. Transport by road at the other extreme cannot be done at a price below the normal outlays of a public carrier, one that is, who is licensed to carry for others and thus has the opportunity of running full both out and home. The costs of carriage by road may be reduced by the construction of motorways and by large (and expensive) improvements to the existing roads. The rebuilding of a highway will certainly increase the convenience brought by the ownership of a motor vehicle. Much depends upon the nature of the improvement and where it is made. The widening of a congested city street, for example, is more likely to be followed by a reduction in costs than the construction of a new motorway through the countryside, however much a new road free from obstruction may add to the ease of the fast

[1] 536 H.C. 1797. 'The right honourable gentleman', said Mr. Holt, concluding this part of his speech, 'gave no economic reason whatever, but finished his speech by saying the best argument for the proposals was that they were a morale raiser.'

[2] Fifth Report from the Select Committee on Estimates 1952–53 Roads; Walker, Beesley and Walters, *Westminster Bank Review*, May 1953, 'Transport Policy and Investment in Roads'.

driver.[1] But the private carrier gains, and so does the private motorist. The second will get there quicker and with less frustration. The first will have his goods carried more cheaply and with greater speed and convenience. We need not expect to see similar reductions in the prices which public carriers by road ask for their services. Their costs will be lower, it is true, to the extent which they work over the new and improved highways. They can always be induced to lower their rates following upon the reduction in their costs if other carriers, private and public, are attracted on to the new road and licensed to work over it. An influx of private lorries can no doubt be looked for, owned by those traders who hope to gain from the improvement. But invasion by the lorries of public carriers is limited by the discretion of the licensing authorities and the addition of public vehicles working over the new or improved road is thus restricted to those 'A' licensees who, under the law, can safely divert their vehicles from other hauls.

The monopoly enjoyed by the holder of an 'A' licence is limited by the costs and convenience of the trader who works or may work his own vans; and circumscribed by the discretion of the licensing authorities, who may grant or refuse applications for public 'A' and limited 'B' carriers' licences. Hauliers granted an 'A' licence are free in law to ply their trade anywhere and in any part of the country. But the Act of 1933 includes the previous conduct of the holder of the licence among the considerations which should guide the licensing authority, and they may now take into account, under Clause 9 of the Act of 1953, the relative prices which are asked for the facilities applied for. Their discretion has thus been enlarged. What use will be made of the enlargement one cannot yet say, for the addition has not yet been tested; but the conduct of the licensee has in the past been held to warrant the refusal of an application for renewal on the grounds that a licence sought in one area has been used in another. Public 'A' and limited 'B' licences alone are subject to the discretion of the licensing authority. The private 'C' carriers licences shall be granted unless the applicant is the holder of a licence which has been suspended or has been revoked. Licences can be revoked or suspended on the ground that any of the conditions of the licence have not been complied with. The Minister has included a clause in the Road Traffic Bill now before Parliament empowering licensing authorities to suspend or revoke 'A' and 'B' licences should they be satisfied that the licensee has been 'persistently charging' for carriage by road 'sums insufficient to meet the cost of rendering those services, and has thereby placed other holders of licences at an undue or unfair disadvantage in competing with him as respects the carriage of goods by road.'[2] These provisions, besides opening the way to the exercise of forensic skill, may impose upon the licensing authority, if unwisely interpreted, some part of the responsibility for managing the carrier's business. The licensing authorities are already required, on the applications before them, to decide whether or not the case has been made for adding to the public facilities for transport. They can now consider the rates proposed by applicants and should clause 35 stand part of the Road Traffic Act, they and their staffs may also be required to review the costs at which business is being done. Who, in these circumstances one is tempted to ask, is the carrier—the man who sets the rate or the authority who determines that the price is insufficient to cover all the outgoings? These powers will certainly limit still further the competition offered by public carriers licensed to run over

[1] The cost of the improvement could be brought home as a price to each user by the charging of a toll. Tolls have this advantage, that the user can be asked to pay a price not less than the worth to him of the highways each time he runs over it. But the raising of a toll, it must always be recognised, diminishes the use which is made of the facility. The new road is built and the improvement undertaken to ease the flow of traffic. The object should be therefore, to get the motorists off the old, congested road on to the new, presumably uncongested motorway. That is to be done, not by asking a toll on the new road but by a levy upon those who persist in using the old!

[2] Clause 35, Section 1.

the public road; and administrative officers forced to embrace so onerous a liability are not perhaps to be envied!

Consideration of the price and of the cost at which carriage can be afforded by rail and road respectively may not be the most important among the factors dominating the competition between them as Mr. Dennis Peel, who speaks from long experience, may be telling us later in the week. Comparisons of cost, however elaborate, even of marginal cost, supposing always that a meaning can be attached to the term in the context of a consignment of freight or of a passenger, do not get us very far. Nothing is to be gained, as I had thought earlier,[1] in the pursuit of some chimerical notions of 'fair' competition between road and rail, particularly when railways are required by law to pay compensation upon a past investment and motor hauliers privileged to work over the public road. It was, I believe, Immanuel Kant who remarked that the better part of wisdom is to know what questions may be properly asked. May I commend that conclusion to my audience and ask them to devote their attention instead to the issue of motor road carriers' licences, public, limited and private, and to the question, really significant now that competition is to be relied upon to control the price of transport, of why indeed there, should be any restriction at all upon the right to carry by road.

APPENDIX

A Piece on Parking

The common law secures to the ordinary man or woman, on his or her lawful occasions, the right to pass over land lying along the Queen's highway. The road, as we know it, besides being put to this historical purpose now serves also as a warehouse for cars not being garaged or parked elsewhere. The Ministry of Transport, the County and County Borough Councils and lesser authorities, succeeding to the jurisdiction of the highway parishes, Justices of the Peace and the turnpike trusts, have expended a great deal of money on paving and widening the old road and in building entirely new roads, particularly in urban districts. These expensive works are demanded by growing population, by rising standards and by the increasing use of cars and other motor vehicles. The highway is dedicated to the passage of the traveller and the right to obstruct must obviously be subordinated to the free flow of traffic. On some thoroughfares parking should be prohibited entirely. In the residential roads remote from the main streets of the town there is little traffic and much room. No real obstruction is created by stationary cars and no reason therefore to place any restriction on parking.

In the side streets of the town itself, however, there are more people wanting to leave their cars than there is space to park. To buy land, to widen the street to provide both for parking and for traffic is impossibly expensive. If parking is not to be prohibited in these areas, then means must be found to allocate the available space. Room is distributed now by the arbitrary practice of allowing the first comer to park after space has been cleared. Only the few in any case can park close at hand. Others must go farther afield. The opportunity to park is a special privilege which might properly be limited to those few who are prepared to pay. A price put on the occupation of space by parked cars wherever the obstruction could be tolerated would, of course, clear the streets of all but those to whom the privilege of parking was worth the price. Official policy in Great Britain is directed towards the provision, by private enterprise, of space off the street sufficient to accommodate the cars wanting to be left in central areas. The price to be asked for the right to park in the side streets of a town should thus exceed the cost of providing space off those streets by an amount reckoned as equivalent to the greater convenience of

[1] See my study of transport in Nova Scotia in the *Reports of the Nova Scotia Economic Council*, Vol. VI, No. 48, particularly pp. 74–86 (Queen's Printer: Halifax, N.S., Canada, 1942).

leaving a car at the curb compared with the trouble of finding a garage or off-street parking lot. Demand at any particular time and place could readily be adjusted to the space available by varying the price and numbers wanting to park could be kept within any bounds which had been determined by fixing upon a price sufficiently high. Anybody unwilling to pay so much could remove his car to the more distant residential streets or better still leave it at home and travel into town by public transport. This has to be provided in any case.

Opponents of the principle of charging for the privilege of leaving a car on the public highway might care to reflect that the motorist wanting to park does know how much the privilege is going to cost him should he be so unfortunate as to attract a summons. The uncertainty could be avoided by the placing of a known price on the occupation of space for a specified time. One who pays for a privilege can be allowed to exercise his rights during the period of his franchise. A part of the present cost of parking (to the community) is undoubtedly the expense of prosecution. It might thus help if constables, on detecting the perpetrator of an alleged obstruction, were empowered to collect on the spot a sum for which the summons (the ticket) could be furnished as a receipt. The amount could be forfeited as bail should the defendant not respond to the summons, returned if the bench were satisfied that no obstruction had been caused nor other offence committed or impounded if judgment should go against the motorist. The legal purist raised on the common law might not be satisfied, but the motor owner, wanting in this year of grace to leave his (or her!) car temporarily by the curb might be glad to know just how much the privilege was going to cost.[1]

[1] Road Traffic Bill, Clauses 15 to 20: Working Party on Car Parking in the Inner Area of London: *Report*, 1953; Day and Turvey, Parking Problem in Central London: *Journal of the Institute of Transport*, July 1954; and on the rough justice dispensed in the some states of the Union, D. W. Brogan, *Manchester Guardian*, May 18, 1956.

OIL AND INDUSTRY[1]

ADDRESS BY A. C. HARTLEY, C.B.E.

PRESIDENT OF SECTION G

To be President of Section G is a great engineering honour, and it gives me particular pleasure as a Yorkshireman that the meeting in my year of office should be held in Sheffield. Little did I think when I attended my first British Association meeting in 1922 in Yorkshire and in my native city of Hull that I should occupy this position today. Naturally, I have looked up the proceedings of the last meeting to be held here, and I see that it was in 1910, the year in which I took my Bachelor of Science degree, and I am very interested to be reminded that the president of this Section that year was my old professor, the Dean of the City and Guilds College, London, Professor W. E. Dalby, F.R.S., for whom I had the highest regard. I am indeed proud to follow him here forty-six years later.

He chose for the title of his address 'British Railways, Some Facts and a Few Problems'. He thus anticipated the title by which our railway system has become so familiarly known in recent years. He began his address by saying, 'It is remarkable how few among us really realise the large part that railways play in our national life.' I might well open this address with the same sentence, only changing the word 'railway' to the word 'oil'. He went on to say how few of us realise that the capital invested in the railway companies of the United Kingdom was nearly twice the amount of the National Debt. This capital he later referred to as having been £1,310 million pounds in 1908.

Compare this railway expenditure with that of the oil industry of the free world, which, according to some recent estimates, is investing each year more than twice that total amount. It is nearly £2,900 million annually. Even this figure is estimated to be increased to an average of £4,000 million a year during the period 1956 to 1965, and again to an average of £6,500 million a year during the period 1966 to 1975. I quote these figures to give you some idea of the present magnitude of the oil industry and the vastly greater part it is being called upon to play in supplying energy to the free world in this pre-atomic age, largely due to the failure of coal to maintain, let alone increase, its contribution.

I have chosen the title 'Oil and Industry' for this address because I want to describe here at Sheffield the essential part which industry as a whole, and particularly engineering, has played during the first century of oil development, and to emphasise the greatly increased demands which the oil industry will make on other industries for plant and services if it is to make available the fuel, lubricants and other oil products in the quantities on which the very existence of the free world may well depend. I will first describe very briefly some of the highlights of this first century of oil, for it is very little more than 100 years since Dr. Young, a Glasgow chemist, first distilled oil from rich Cannell coal in Scotland and so produced the first mineral oil, and it is less than 100 years since Col. Drake in 1859 discovered the first natural-oil well in the United States of America. Keen demand for his products soon resulted in Dr. Young working through the available supply of rich Cannell coal, but, with the help of engineers, he improved his plant and methods so as to meet economically the increasing demand by distillation from oil shale, a raw material which, although cheap and abundant, gave only a fraction of the yield obtained from Cannell coal. These developments led to the establishment of the Scottish shale-oil industry, and it is interesting to note that Dr. Young took out

[1] Address delivered at the Sheffield Meeting of the British Association on August 30, 1956.

patents in which he described the processes of distillation, cracking and refining by chemical treatments, many of which are in use today. By 1865, further engineering developments and improvements in the plant used had led to very rapid growth and there were no less than 120 shale-oil works in operation. Then, however, oil products from the United States, where there had been even more rapid development following the discovery of the first natural-oil well already referred to, began to compete in the European market, being much easier to produce and refine than oil from shale, and the consequent rapid fall in prices led to the abandonment of many of the shale works. Some, however, fortunately survived by applying the closest attention to the development of their processes and using to the full each advance in engineering to improve their retorting and refining plant. The old horizontal iron retorts were replaced eventually by batteries of vertical steel refractory lined retorts burning the fixed carbon in the spent shale as fuel and capable of handling thousands of tons of shale a day.

Civil, mechanical and electrical engineering all contributed to the full in the development of economical methods of mining, handling, crushing and retorting the shale and in discovering, producing and transporting natural crude oil and refining and marketing the products, thus paying back the debt they owe to the oil industry for providing the fuel and lubricants that made their own engineering developments possible. The steady and continuing development of the shale-oil industry, in spite of all its difficulties, has been of the greatest importance in providing experience for British engineers, which has enabled them to play a very large part in the rapid development of the natural-oil industry in many parts of the world following Col. Drake's discovery well.

Within ten years of Col. Drake's discovery, crude oil was being produced in Rumania, Russia, Italy and Canada, and by 1900 in Poland, Japan, Germany, India, the Dutch East Indies and Peru. Discoveries in Mexico, the Argentine, Trinidad and Persia had followed by 1911. Engineers and engineering plant and services of all kinds have been used by the industry in this world-wide search for oil. Throughout its whole history, the oil industry has been amongst the foremost in encouraging the development of promising new engineering discoveries by placing early orders for their use and helping them through their teething troubles, and by meeting in so many cases their need for improved fuel, lubricants and other oil products, which have, in their turn, led to yet further developments.

I will now endeavour to describe some of these under the headings of the various phases of the industry.

Oil Exploration

It is now generally accepted that the original source of oil is organic in nature and that it consists of the bodies of marine organisms and possibly of some plant life formed at the bottom of seas, lagoons, etc., and preserved by the steady deposition of fine grain sealing formation such as mud, clay, lime, etc. There is still, however, much conjecture as to the chemical processes by which they have been changed into oil and gas, and whether this has been by heat and pressure or by the effect of hundreds of millions of years irrespective of heat and pressure, or possibly even by biological processes. The important fact remains, however, that unlike coal, which is solid and remains where it is formed, oil is liquid and may migrate considerable distances laterally and vertically, and only provides a prospective oilfield if it is trapped in reservoir rock or sand, sealed in by impermeable cap rock.

The civil engineer was very early on the scene to play his part in oil exploration by surveying, map-reading, road-building and providing means of access and subsistence to enable geological surveys to be made. In recent years, these ground surveys had been greatly helped by air reconnaissance, air photography and photo-geological interpretation. Mechanical and electrical engineers and engineers from

many of the newer branches of engineering soon entered the field and are playing an increasing part in contributing the improved methods now used for supplementing the efforts of the geologist and making more certain the location of prospective oilfields. They have assisted geophysicists in the development and use of gravimetric, magnetic, electrical and seismic methods of surveying. These depend upon highly sensitive instruments to measure slight variations in gravitational forces of the earth, in the intensity of the magnetic field, in electrical conductivity and in the velocity of transmission of seismic waves in the earth.

The use of one or more of these geophysical methods often establishes the possible existence of favourable underground conditions, but cannot indicate whether oil has ever been trapped in the structure and, if so, whether it remains there now. The only method of proving an oilfield is by drilling wells, but further developments in geophysics are assisting in the location of test wells so that fewer dry holes are now being drilled.

Oil-Well Drilling

The only equipment available to Col. Drake when he drilled his first well in Pennsylvania in 1859 was that normally used for drilling water-wells, and it is fortunate that he had only to reach a depth of 69 ft. It says much for the pioneers who produced the early drilling rigs, largely from rough-hewn timber, that there were traces of many of their ingenious devices in the design of drilling rigs for the next sixty years or more. Drilling during that period was by the cable tool method, in which a heavy steel bit of the chisel type was suspended by a manilla rope in the earlier days and later by steel-wire cable, motion being imparted to it by a crank and beam which utilised the stretch and spring of the rope or cable to produce an impact on the formation somewhat like the cracking of a whip. The chippings were baled out after 5 or 6 ft. had been pulverised, and a good day's progress was usually of the order of 15 to 25 ft. if the going were good. Mechanical engineering developments just before the turn of the century led to the adoption of the rotary, or mud flush, system of drilling and real progress began to be made. This system, as its name implies, depends upon the more orthodox method of rotating a bit of special design which cuts the formation with a combination of a cutting and paring action imparted by the cutter wheels revolving in the bit head. The bit is connected to screwed drill pipe rotated at surface by a special rotary table, a continuous flow of mud being pumped down the drill pipe and up the annular space, cooling and lubricating the bit and bringing the cuttings to the surface. The mud is loaded with barytes and other materials to specific gravities of 1·25 or more in order to counterbalance the pressure of the oil in the formation and to prevent the oil gushing to the surface when the well is drilled.

A special hydraulic packing gear has been devised to enable drilling to be carried out under pressure, and it is used in the case of very deep wells where the weight of mud alone would not counterbalance the oil pressure. It has been necessary to develop throughout the years improved drilling techniques to enable ever deeper wells to be drilled and deeper oilfields to be discovered. At these depths, higher pressures are encountered, and reservoirs having rock pressures of more than 6,000 lb. per sq. in. can now be explored without difficulty. The very special problem of drilling three miles or more below the surface called for a highly specialised design of casing, drill pipe, rotary tables and particularly of lifting gear, which is the key to the operation. The necessity for very rapid hoisting speeds, so that the drill pipe can be drawn from the hole for changing bits and lowered again with the minimum loss of time, has produced a special design of hoisting gear which has no equal in any other branch of mechanical engineering. The handling of high pressures both in mud, oil and gas calls for very special designs in valves and surface fittings, as well in oil-well casing.

The requirements of the oil industry have led to the development of special types of steel in order to produce the toughness and high tensile qualities required at these great depths, where dimensions permit of only the minimum factors of safety. Steam was used extensively in the early days of drilling to drive the draw-works, rotary table and mud pumps, and it is still the most satisfactory power because of its flexibility. It cannot, however, be used economically in oilfields where water is extremely scarce or unsuitable for boilers. In such conditions, the internal combustion engine and electric motor drives have been increasingly used. The necessary flexibility to enable steady pulls to be maintained by the draw-works on the drill pipe and steady pressure on the mud, etc., has been obtained by fitting hydraulic couplings. Drilling by electric motors has usually been adopted where a large number of wells have had to be drilled in a fairly restricted area and where it has been possible to obtain sufficient electric power.

The depth of the early wells was rarely more than 3,000 ft., but wells are now regularly drilled to 17,000 ft., and in some cases even to more than 20,000 ft.

The industry has used each advance in the design of new tools for fishing in case of 'twists off' of drill pipe, of new methods of coring and of testing wells. Production in tight formations has been increased by shooting and by acid treatments. More recently, methods of electrical logging have been used to survey wells before the casing is lowered and have provided valuable geological information, and in many cases have done away with the need to core and have greatly speeded up operations.

Oil Production

In the early low-pressure oilfields, relatively small quantities of gas came out of solution when the oil was reduced to atmospheric pressure, and this gas separation could be allowed to take place in tanks working at atmospheric pressures with very little effect on the quality of the crude. In the deeper oilfields now being produced, the pressure of the crude oil in the reservoir may exceed 5,000 lb. per sq. in., and flowing pressures at the top of the well may be more than 3,000 lb. per sq. in. Natural petroleum gas in proportions varying from 2000 : 1 to 4000 : 1 by volume is dissolved in the oil. If these high pressures were released in a single stage, large quantities of light oil fractions, of great value as a constituent of petrol, would be carried away with the gas. The flow of oil is therefore directed, after a preliminary separation of the leanest gas at the well head, to a series of heavy steel separators, each being at a lower pressure than the one before and ranging from about 1,500 to 15 lb. per sq. in. The gas is taken off at each stage, and the crude oil is then delivered to flow tanks from which it is pumped down pipelines to the refinery or to the crude-oil loading terminal. The gas is used as far as possible in the oilfields and in the pumping stations and refineries and is processed for production of special products.

Crude Oil Transportation

Oilfields are usually found a considerable distance from the refinery or crude-oil loading terminal site, which must necessarily be alongside deep water to enable products and crude oil to be loaded into ocean-going tankers for delivery to the world's markets. Oil was for many years transported from the wells in oak barrels, but proposals were made as early as 1861 for a wooden pipeline 4 in. in diameter and 6 miles long. The first successful line was made of cast-iron pipe, 2 in. in diameter, with screwed joints. It, too, was 6 miles long, and had steam-driven pumps which delivered about 70,000 gallons of oil a day. Further lines were built during the next year or two, and by 1878 a 6 in. line, 125 miles long, had been built across the Alleghany Mountains. By 1910, about 20,000 miles of line were in operation, and

this was increased to 70,000 by 1920, and to 112,000 by 1930. The use of cast-iron pipe was replaced by lap-welded steel pipe, and later by seamless steel pipe. Diameters had been increased to 16 in. by 1939, and screwed joints had been replaced by welded joints, first with swelled ends and inner rings, and later by plain butt joints.

Problems of expansion and contraction had quickly asserted themselves in the earliest days, and lines buried in the ditch during the hot summer weather suffered from pulled-out joints in the next cold weather. Similarly, lines laid in shallow ditches in cold weather expanded and threw themselves out of the ditch in the next hot weather. These difficulties were overcome by crowding an excess of pipe into the ditch with sufficient cover to hold the pipe in position, but serious corrosion problems were the next difficulty. An extra thickness of steel was first used as a corrosion allowance, but this only postponed the trouble. Methods of picking priming, wrapping and enamelling the pipe were then developed to provide protection against corrosion, and means have been devised for testing the soundness of this protection.

The alternative method of laying and supporting lines above ground has been successfully used in Iran, where there is no atmospheric corrosion. There was first a tendency for lengths of some miles to move in one direction under the influence of the daily expansion and contraction due to the variation of 100° F. in day and night temperatures, but this was overcome by making a sharp bend in the pipe of a few degrees alternately in each direction every 70 yards or so, thus zigzagging the pipe across country.

During the 1939–45 war, a 24 in. pipeline, 1,352 miles long with twenty-six pumping stations, was built in America to carry Texas crude oil to the industrial areas of New York and Philadelphia, and this was followed by another pipeline, 20 in. in diameter and 1,475 miles long, for conveying finished products. These lines worked at 700 lb. per sq. in. pressure and were made necessary by the severe losses being inflicted on tankers carrying oil along the Atlantic seaboard, but, apart from these losses, they proved so much more efficient in the use of steel, power and men than tankers, that they quickly led to proposals for more large-diameter lines. Every inch in diameter of line gives an appreciable reduction in capital and operating costs, but brings new problems to the mechanical engineer to solve. Seamless pipe cannot be made at present in diameters larger than 26 in., and there is great difficulty in maintaining reasonably uniform wall thickness. Pipe of uniform wall thickness has been made for many years by rolling single steel plates in the form of a circle and then fusion welding the longitudinal seam. The pipe is then subjected to an hydraulic pressure sufficient to stress the steel beyond its yield point and expand the pipe into a die. The pipe is thus made truly circular and straight, and the yield point of the steel is raised.

The oil industry, and particularly that part of it laying gas-lines, has been quick to take advantage of the higher permissible working pressures of this type of pipe by reducing the wall thickness and making great savings in steel. This fusion-weld process involves the use of very heavy plant which limits the diameter of the pipe produced to about 26 in., and engineers have made a valuable contribution to the oil industry by adapting the submerged-arc welding process for the reliable production of welded and expanded pipe of up to 36 in. diameter. This increase in diameter has enabled great savings to be made in the capital and operating costs of pipelines and has led to the construction of the 30-in. diameter Trans-Arabian pipeline, 1,000 miles in length, from the Persian Gulf to the Mediterranean, the 30 in., 32 in. Iraq Petroleum Company's line from Kirkuk to Banias, also on the Mediterranean, and many large-diameter lines in North and South America and in Canada, notably the Trans-Mountain pipeline, more than 700 miles long, from the Alberta oilfields to Vancouver.

The oil industry has encouraged the development of specialised earth-moving, ditching and back-filling machines for preparing the right of way and the trenches, and side boom tractors, cleaning and priming and wrapping machines for handling and protecting the pipe against corrosion. Civil and mechanical engineers have made great contributions to these developments, and more recently electrical engineers have contributed to the development of cathodic protection, which is now almost always applied to pipelines to deal with the inevitable faults or damage to coatings called 'holidays'. In this method, current is drained from the pipelines either by coupling them to magnesium anodes or, when an electric supply is available, by using transformers and rectifiers and ground beds. Experience has shown that, when correctly applied, complete protection can be given to pipelines even when considerable damage has been suffered by the coating, but in these cases it is at the expense of increased consumption of anode or current.

Reciprocating pumps of the duplex and triplex double-acting ram type were almost universally used for oil pipelines operating at pressures of 600–800 lb. per sq. in. in the earlier days, and were driven by steam, compression-ignition engines or electric motors according to circumstances. In 1916, multistage centrifugal pumps driven by steam turbines were installed in the Persian pipelines and proved to be very flexible and capable of running very long periods without shut-down. In the case of desert lines where water is not available, centrifugal pumps have been driven by compression-ignition engines through speed-up gears, and heat-exchangers have been installed to enable the circulating water of the engine to be cooled by the oil which is being pumped. In the early centrifugal pump installations, each pump operated in parallel, developing the full line pressure, but more recently, pumps have been arranged in series so that the full throughput passes through each pump in turn and each adds its proportion to the full pressure. More efficient, flexible and simpler installations have thus been achieved.

The pipelines deliver oil to storage tanks in the refinery or the crude oil terminal tank farms, where all-welded steel tanks are now the common practice. They are usually of 10,000 to 16,000 tons capacity, but some of more than 20,000 tons capacity are now being built. These are 168 ft. in diameter and 48 ft. in height. They are usually surrounded by bunds capable of containing the oil should the tank be damaged. Fixed roofs are used for heavy, non-volatile oils, but steel roofs floating on the oil are now used for crude oil and for light oils in order to reduce loss by evaporation and to reduce fire risk. Great care is now taken to earth all parts of the tanks and protect them against lightning. Cathodic protection is frequently applied to prevent corrosion of the tank bottoms, and tanks are carefully insulated from the pipelines to prevent increased corrosion if they should become anodic to the pipeline system. The pipelines so far referred to have been for handling crude oil, and pipelines for supplying finished refinery products to the marketing depots will now be described.

Refined Products Transportation

Pipelines for refinery products have been developed chiefly in the United States, where refined products are handled in larger quantities than elsewhere, and there are now 25,000 miles of products lines in operation. At first, a different line was used for each product, but during recent years it has become almost universal practice to pump aviation and motor gasolene, kerosene, gas oil and light heating oil through a single line, and in some cases butane and propane and heavy fuel oil have also gone through one line. Plugs of water or mechanical separators were at first put between the different products, but experience has shown that provided the velocity of the oil in the pipe is kept well into the turbulent region, and if the sequence of products is carefully chosen, the amount of co-mingling is tolerable and products can be kept to specification when delivered to the depots. Inhibitors,

either water or, more recently, oil-soluble, are introduced into the line at the first pumping station and their amount is regulated to prevent corrosion inside the line. It has been found that the friction factor of the line can be kept low by these means. Facilities are always provided for running scrapers through the line.

Products pipelines of diameters from 6 in. to 20 in. or more have been installed and they are operated at pressures of up to 1,000 lb. or 1,200 lb. per sq. in., chiefly by centrifugal pumps. These are almost always designed for series pumping in each station, and the lines are operated throughout from the initial station, through the boosting stations as a closed or tight system, without the product going in and out of tanks. Reliable displacement meters have now been produced commercially and are used wherever oil is put into or taken out of the system. Provision is made for the regular checking and calibrating of the meters under the working conditions of temperature and pressure, and the passage of the interface between any two products can be accurately forecast to enable the operators to carry out the necessary switching operation at intermediate take-off points and at the terminal into the appropriate tanks.

Standby pumping capacity is installed at the initial station, but frequently not at the intermediate boosting stations, because in series pumping the spare at the initial station can raise the pressure to some extent and help the booster. Electric motor drives are generally preferred where an electricity supply is available, but compression-ignition engines are frequently used with speed-up gears of ratios of up to 10 : 1. Centrifugal pump speeds of 3,500 r.p.m. are generally used, but in some cases pumps with speeds as high as 5,000 r.p.m., taking 2,500 h.p., have been installed. There has recently been a tendency towards unattended pumping stations, both electric and engine driven, and the sequence of starting and shutting-down operations is automatically timed and controlled and is set in motion and monitored by means of telephones and teleprinters hundreds of miles away. During operation, these stations automatically report to the central control room readings of suction and delivery oil pressures, speeds, temperatures, etc., at prearranged intervals.

Products pipelines are almost always buried in order to avoid undue variations of temperature, and therefore of expansion and contraction of the oil, which makes accurate forecasting of the position of the interface more difficult. Where sufficient movement of oil products in one direction is required, bulk pipeline transport has proved to be more economical than rail or road transport, even in average terrain, and much more so in difficult or mountainous country, and many lines are at present under construction or in the design stage. One interesting case is the 10 in. Trans-Iranian products pipeline to be built to take Abadan products to Teheran with a first-stage capacity of 1,000,000 tons a year and a second stage with extra pumping stations of 2,000,000 tons a year. This line will be more than 600 miles long and will cross difficult mountain ranges at 6,200 ft. elevation, through which at present there are only difficult roads and a single-track railway, whose capacities are urgently needed for general traffic. Branch lines will later be added.

During the war, military rather than economic considerations were all-important, and even where the quantities of products required to be moved in one direction were relatively small, pipelines of small diameter were laid to avoid the use of packages or bulk road or rail transport and to ensure regular supplies. Indeed, difficulties in ensuring regular supplies by tins, barrels or jerricans, with losses of more than 50 per cent. in extreme cases, led to the laying of pipelines to follow as closely as possible the advance of the armies in North Africa and elsewhere in 1941. These were 'victaulic' jointed steel lines of 4 in., 6 in., 8 in., and 10 in. diameter laid on the ground, or buried lines with other types of joints. Petrol or diesel engine-driven pumps were installed to boost the oil every 10 or 20 miles or

so. The success of these pipelines and the increasing fuel demands of the modern mechanised army led to the requirements stated by Lord Louis Mountbatten in April 1942, and referred to by the Rt. Hon. Geoffrey Lloyd, Secretary for Petroleum, for a cross-Channel petrol pipeline to supply the Allied armies when the re-invasion of the Continent took place. This involved the laying of two types of 3-in. pipe which would have been completely uneconomic in peace time, but which eventually supplied 172 million gallons of motor spirit across the Channel with the loss of only 1·1 per cent.

'Operation Pluto' (Pipe Line Under the Ocean) has been described in my Evening Discourse at the Belfast meeting in 1952, and it will suffice here to say that one type of pipe was of more or less standard submarine telephone cable construction with a lead pipe of 3 in. internal diameter, wrapped with layers of paper and cotton tape, followed by four overlapping layers of steel tape to enable working pressures of 1,500 lb. per sq. in. to be used. Then followed layers of jute and of steel armouring wires and of more jute. The steel tapes were wound in right-hand lays and the steel armouring wires in left-hand lays to balance the cable and keep it from moving when pressure was applied. Four merchant ships, two of 10,000 tons, able to handle 100 miles of cable in one length, were fitted with cable storage tanks and cable-laying machinery for laying the main lengths, and six Thames barges were equipped for laying the shore ends. Mechanical joints were designed for coupling the main lengths and shore ends at sea. Cable pipes were laid successfully by ordinary cable-laying methods at speeds of up to 6 knots, and eleven pipes were in operation from Dungeness to Boulogne until well after VE-day.

The other type of pipe was 3 in. diameter steel tube of 0·22 in. thickness. Standard 40 ft. lengths were flash-welded together into 4,000 ft. lengths at specially installed plant at Tilbury and stored in racks between the plant and the dockside. These lengths were then welded into one continuous length and neatly wound by holding them back with a 3-ton pull on to a floating drum or Conum, which was rotated to pull the pipe round its 40-ft. diameter while floating in the water. Six Conums were built and could carry 90 tons of 3-in pipe on their 60 ft.-wide drum. They were then towed across the Channel by powerful Ocean Rescue type tugs, while the steel pipe peeled off and sank to the bottom. Difficulties were at first experienced in pulling the steel pipe ashore, but these were avoided by using the lengths of flexible pipe cable coupled to the steel pipe for the shore ends. Six of these lines were laid, and their time in service exceeded that estimated, having regard to their vulnerability to erosion. Well-camouflaged compression-ignition driven reciprocating and electrically-driven pumping stations were installed at Sandown and Shanklin in the Isle of Wight to pump across to Cherbourg, and at Dungeness to pump across to Boulogne. The pipelines were all run in parallel, and the pressure was varied from 900 lb. per sq. in. to 1,500 lb. per sq. in. to deliver the quantity of motor spirit ordered each day from Headquarters in Brussels. The total available daily capacity was approximately 1½ million gallons. These lines had no commercial value after the war and were pulled up to salvage the lead and steel.

It will be seen that there has been great progress in the transportation of oil during the past century, and it is certainly true that there are still great opportunities for engineers to develop and improve plant and techniques and so enable the rapidly increasing quantities of oil required to be transported.

Oil-Refining

Refining is a combination of physical and chemical operations designed to convert crude oil into a series of fractions or cuts and to treat these raw products chemically to remove any undesirable impurities, such as sulphur compounds and unsaturated hydrocarbons. In the early days, separation was achieved by distillation in simple

batch stills, which produced a few impure fractions, such as petrol, paraffin and fuel oil. A simple treatment was used to improve the colour and odour, and the products were then marketed. Demands for better products in ever-increasing quantities led to the development of continuous shell still distillations units and later to pipe stills with fractionating columns. These methods produced straight-run motor spirit up to the limit of that naturally available in the crude. In the 1920's, demands arose for a much improved motor spirit, and processes of thermal conversion or cracking were developed. In recent years, further processes, called thermal reforming, catalytic cracking, polymerisation, alkylation and isomerisation, have been developed. These processes enable the proportions and qualities of the various fractions to be adjusted to market requirements. Vast plants requiring power stations, water pumping plants and services on the scale of a large provincial town are required. There is no space now to deal further with this branch of the industry, but some of the products will be referred to later.

Oil-Loading

Reference will now be made to loading of oil at the seaboard to sea-going tankers. Until the outbreak of the war, tankers were rarely more than 16,000 tons capacity and required a depth of about 32 ft. at low water. Since the war the size of tankers has been increased to 32,000 tons or more. These require a minimum depth at low water of 40 ft. Where this depth could be obtained in a river close to a suitable site for a loading terminal, light jetties were generally used to carry the oil loading pipes and more substantial dolphins were constructed against which the tankers could be berthed. In the early development of new fields, where river loading facilities were not available, sea loading lines were pulled from the shore, in some cases to a distance of two or three miles, to supply tankers moored at buoys where the depth of water was sufficient. These loading lines were, until fairly recently, 10 or 12 in. in diameter, but greater experience in laying sea lines has made it possible to use diameters of 20 or 24 in. with greatly increased throughput.

When an oilfield has been established and greatly increased throughput has to be dealt with, the very great capital cost of constructing a deep-sea jetty becomes justified by the savings that can be made by a quicker turn round of the large modern tankers and the reduction in days lost due to bad weather. In the case of the Kuwait Oil Company, a jetty was built 4,200 ft. out to sea, with accommodation for six tankers and two cargo vessels. This has recently been adapted to take eight tankers, and more sea loading lines have been pulled. The jetty has been fitted with gravity-fed fenders in groups of three, consisting of steel cylinders, 21 ft. long and 6 ft. in diameter, filled with concrete and weighing 43 tons. The cylinders were slung by links in such a way that the impact of a tanker when berthing or riding in a choppy sea was absorbed by pushing them back and upwards against the force of gravity. They have been most successful in enabling operations to be continued without damage to tankers or jetty during bad weather. Tanker loading rates of 3,000 tons an hour and more are now common practice.

Oil Distribution

The crude oil and products shipped from overseas oilfields and refineries in ocean-going tankers are discharged at ports or special oil terminals where jetties having an adequate depth at low water can be installed, and the oil is pumped out of the tanker by their own pumps to tanks as near the jetty as possible. Thereafter, oil is distributed by pipeline, road or rail tank cars, or by small tankers or barges for coastwise and inland traffic. There is no space in this article to describe the distribution of products to the smaller depots and eventually to the service pump or to the containers.

Oil in War

Reference has been made under 'Refined Products Transportation' to Operation Pluto as a special wartime activity of the oil industry, which was sufficiently developed by 1939 to play a very full part in the war. Perhaps the greatest achievement of the industry was the production of aviation petrol, of the quality and in the quantity required to meet the ever-growing demand of the Allied Forces. Plans for the distribution of motor spirit and aviation spirit to the Forces had been made in the years immediately preceding the war and a large number of plants had been built for making 4-gallon non-returnable tins. These tins had for many years, particularly in the East, been successfully used commercially, but they had always been protected by placing them in pairs in wooden crates, or singly in cartons. It soon became impossible to clothe the tins in this way owing to shortages in supplies of wood and cartons, and experience soon showed, particularly in North Africa, that very serious losses were incurred in handling supplies in this way. Modern mechanised armies demand fast and constant supplies of petrol and it soon became apparent that methods of delivery must be improved. The Germans developed a very well designed substantial 4-gallon container which was christened the 'jerrican' and which was used as a returnable container. It had a particularly good filler and pouring cap and was quickly reproduced by the Allies after the first samples had been captured. These replaced the non-returnable tins, which had been christened 'flimsies', and reduced losses.

Demands were ever increasing, and it became apparent that bulk supplies of oil by pipeline must be made as near the front line as possible. Light steel victaulic jointed pipes and portable petrol or compression-ignition engine-driven pumps, as referred to above, were therefore supplied in quantity.

Apart from the normal contribution of the oil industry to the war effort, which enabled official reports after the war to record that 'the planning and execution of no single Allied operation was prevented or even delayed by lack of petrol, oil and lubricants where and when wanted', the industry made great contributions in special ways. They were largely responsible for the development of flame weapons, the most effective of which were the Wasp, made by equipping Bren-gun carriers, and the heavier Crocodile, comprising a Churchill tank towing, by means of a most ingenious mechanical joint, a 6-ton 2-wheel trailer, carrying the special flame-thrower fuel. The mechanical joint, in addition to providing for universal freedom of movement, had to transmit the controls and the fuel under high pressure. Two troops of Crocodiles landed in Normandy on D-Day and they took part in practically every succeeding action, having by the end of August firmly established themselves as battle-winners. On September 26, 1942, the Prime Minister, knowing the heavy losses incurred by the very large number of aircraft engaged in night bombing operations when attempting to land at home airfields under mist or fog conditions, addressed a personal Minute to Mr. Geoffrey Lloyd, the Secretary for Petroleum, phrased as follows: 'It is of great importance to find means to dissipate fog at aerodromes so that aircraft can land safely. Let full experiments to this end be put in hand by the Petroleum Warfare Department with all expedition. They should be given every support.' A team, largely drawn from the oil industry, studied reports of all previous attempts to clear fog and decided that the best approach to the problem was to develop means for generating heat along each side of the runway, sufficient to raise the temperature of foggy air by 7° F., experiments having shown that crystal-clear visibility would result. Eventually, burners were designed which, when placed in two parallel continuous lines, 50 yards on each side of the normal 50-yard runway, would burn 20 gallons of petrol each yard each hour when supplied by converted N.F.S. fire-pumps at 100 lb. per sq. in. It was soon proved that these would clear the densest fog, whether stagnant or drifting. This operation became known as Operation Fido. Seventeen airfields were

equipped in England and France and assisted the landing of 2,500 bombers under misty or complete fog conditions. Fido's greatest achievement was to enable Bomber Command to keep up its attack at the time of the Ardennes offensive in December 1944, when almost the whole of their airfields were covered with fog for several days.

Growth of the Industry During the Past Sixty Years

As has already been stated, the main market in the earliest days of the oil industry was for lighting oil or kerosene, and though the widest possible fraction of crude was used, the refiner was often obliged to burn off surplus light spirit for which he could find no outlet. This was the position until about sixty years ago, when the internal-combustion engine was first used to any considerable extent and so created a market for the surplus light spirit. During this period, oil consumption has grown from 8 million tons a year to 700 million tons a year, that is, more than eighty times. During the same period, coal consumption has increased from 500 million tons a year to 1,700 million tons a year, an increase of a little more than three times. Put in another way, the average growth in the use of oil has been $6\frac{3}{4}$ per cent per annum and of coal has been only 2 per cent.

Present and Future Position of the Oil Industry

The early history of some of the developments during the past century have now been described, and you will not be surprised to know that it has recently been said that the oil industry, if not yet the leading industry in the world, is very close to it, and you will readily believe, from the rate of growth which has been described above, that it is destined to have a clear lead over all other industries within a few years. Until atomic energy is developed on a vast scale—and it is universally agreed that this cannot happen for several decades—the world will remain critically dependent upon an abundant supply of oil products being readily available at the places and in the forms required. Industrial expansion cannot otherwise be maintained, nor standards of living protected and improved. The magnitude of the task before the oil industry becomes clear when the full significance of the whole economic world activity is examined.

The increase of the world population goes on steadily in spite of wars, and other calamities. In 1900, the population was 1,500 million; by 1950 this had grown 2,500 million, and it is estimated that it will rise to over 3,500 million by 1980 or soon after. An extra 1,000 million people must therefore be fed, clothed, housed, found employment, moved from place to place, etc., in twenty-five years' time. Increasing population leads to increased industrial development and so to increased demand for world energy. The world consumed two and one-third times more energy in 1955 than it did in 1920, and the effective consumption of oil fuels has increased almost sevenfold during these thirty-five years. Thirty-five years ago, oil only supplied one-seventh of this energy, whereas it is now supplying more than half of the effective requirements. More coal was used thirty years ago than it is today, and coal's relative contribution has gone down from five-sixths to much less than one-half of the world's energy requirements. Hydro-electricity supplies a relatively small percentage, increasing from 3 per cent in 1920 to 7 per cent in 1955.

It is likely that atomic energy will be developed at least as quickly in Great Britain as anywhere else in the world, but the official estimate is that by 1975 it will only produce the equivalent of about 40 million tons of coal. The present consumption of energy in Great Britain in all forms is the equivalent of about 250 million tons of coal a year, and it is estimated that this will have risen by 1975 to about 400 million coal tons. It appears, therefore, that at the best atomic energy cannot by that date provide more than 10 per cent of our needs, and the average for the

world will be much lower. In fact, it seems unlikely that atomic energy will be making as high a percentage contribution in 1975 as hydro-electricity is now making. A recent estimate of the free world's consumption of oil in 1955 is 695 million tons, and it is estimated that this will have risen, even making full allowance for any possible contributions by coal and atomic energy, by about 70 per cent to 1,200 million tons a year in ten years' time, and to at least 1,600 million tons by 1975.

This actual growth in the use of oil during the last 100 years, and the estimates for further large increases, is not due to its price, because, on a British Thermal Unit basis, it is almost always more costly than coal. The growth in the use of oil is due to the fact that the industry has always been prepared to spend large sums of money in research and development into new processes in order to provide products best suited for each individual use. This is in direct contrast to the policy of the coal industry, which, with very rare exceptions, has continued to sell what is essentially a raw material.

Modern methods have enabled the kerosene previously used almost entirely for lighting, now to contribute largely to the production of aviation turbine fuel, vaporising oil for agricultural tractors, and to some extent, to the special boiling-point spirit, such as white spirit, used for solvents in the paint industry and for dry cleaning, and more recently for feed stock for petroleum chemicals. The light spirits previously burnt produce the motor spirit used in all petrol engines. The gas produced from straight distillation of the crude is now synthesised and blended with cracked spirit produced by residue cracking processes to provide the aviation spirit required for civil and military piston-engined aircraft. A large market has more recently been developed for using propane and butane as bottled gas for use in caravans, boats and elsewhere when a piped gas supply is not available. It is also used for the cutting of steel.

The next heavier fraction to kerosene is gas oil, and this produces fuel for the diesel engines used on rail and road, and for marine and stationary purposes. Very recently, it has been used by the coal-gas industry for the enrichment and augmentation of gas produced from coal. The straight-run residue remaining from the distillation of gas oil, kerosene and light spirit from the crude has long been used as fuel for ships, both naval and mercantile, and is finding an increasing use in stationary power-plants, steel furnaces, industrial heating plants, and other such uses.

Residue oil has long been processed for producing lubricating oil, and in recent years gas oil, paraffin wax, lubricating oils and bitumen have all been produced from the fuel oil by vacuum distillation processes. The gas oil so produced is used to augment supplies of straight-run gas oil, and the paraffin wax has long been used for candles and, more recently, for waxed papers for electrical insulation, and many other uses. Bitumen (asphalt) has long been used for road making and repairing and for waterproofing. Very much work has been done in producing grades of bitumen best suited for the climatic conditions under which they are to be used and they have played a vital part in the development of highway systems.

If the oil industry had not made all these oil products available, it would have been impossible to develop the internal-combustion engines now so widely used, and road transport and aviation as we know them today would have been impossible.

It has been seen that oil is a very large-scale industry embracing a great range of activities: the finding, production and transportation of the crude oil, refining it, and manufacturing a wide and ever-growing range of finished products, transporting these by land and sea and delivering them to the consumer or retailer everywhere. In carrying out these oil activities, it also has to carry out vast programmes of road-making, housing and ancillary services, health and medical services, education, and co-operate with local governments.

Its research programme is helping in the development of the petroleum chemical industry, which produces solvents, detergents, plastics and synthetic fibres,

synthetic rubber, agricultural chemicals, glycerine and many other products—for example, disinfectants, anti-freeze compounds, and a whole range of pharmaceutical products.

World Oil Reserves

You will be anxious to know whether world oil reserves will be sufficient to support this great and expanding industry. When considering this, clear distinction must be made between what the industry calls proven reserves on the one hand and total estimates of ultimate recoverable reserves on the other. Proven reserves, only taking into account the known quantities of oil in the various oilfields already discovered and which can be produced under existing conditions of present-day methods of extraction, are currently estimated to be 25,000 million tons, and these would be exhausted by about 1975 according to the annual consumption figures already quoted. When we take into account the recoverable reserves, we see quite a different picture. These include amounts of oil which are known to exist outside the so-called proven areas and also large amounts still awaiting discovery in the very extensive potential oil-bearing areas of the world.

It is rather significant that every time this estimate has been made, it has been higher than before. In 1945, the total oil reserves of the world were estimated to be 70,000 million tons; the estimate made in 1954 places the figure at 85,000 million tons, and both these estimates exclude the Continental shelves. Recent data suggest that if these are included, and with the present knowledge of fields discovered under the sea, the estimate might well be 140,000 million tons. The actual production experience of the industry from year to year has been that the rate of discovery has been, and continues to be, considerably higher than the rate of withdrawal. It is well realised that this cannot go on for ever, but it may be taken to be reasonably certain that we can safely expect the ultimate recoverable quantities of oil in the ground to be sufficient to enable the increasing demands of world economy to be met until it moves into the age of atomic power. This position is due very largely to the vast discoveries in the Middle East, which now has about 70 per cent of the world's proven reserves compared with the U.S.A., which has only 17 per cent.

Oil and Other Industries

Oil is very closely linked with many other industries, notably with steel. The almost uninterrupted increase in the output of Britain's steel industry ever since the war has been in no small measure due to the greater use of fuel oil. The oil industry, being one of the largest users of steel for its tankers, pipelines and oil equipment of all kinds, has been one of the chief beneficiaries of the larger supplies that have become available and has used them to increase its output of oil.

The steel industry is becoming an increasing user of oil fuel in smelting, metal melting, welding, and bending and forging of steel bars because of the close control of temperature that can be obtained by automatic means, the freedom from ash, and the greater output of better-quality work which results. The steel industry also uses petroleum for cutting and quenching purposes.

Oil has played a great part in the development of agriculture by making mechanisation possible. Before the 1914–18 war, very few tractors were available, but by the early 1920's their number had increased rapidly, particularly in the U.S.A., where there were half a million by 1925. At the outbreak of the Second World War, there were 2,500,000 tractors in use throughout the world, and by 1952 about 7,000,000. Oil alone has made possible the light general-purpose tractor which has done so much to speed up production and eliminate much of the toil previously associated with agricultural practice. In addition to drawing an untiring plough capable of working round the clock if necessary, it will load manure, distribute

fertilisers, pull trailers and harrows, and use some sixty interchangeable implements for planting, mowing, sawing, post-hole drilling, etc., etc. With a power take-off, it turns from a mobile to a stationary power unit.

Petroleum also assists agriculture by improving insecticides and pesticides by providing kerosene and other light oils instead of water as carriers for toxic substances and so making possible a much greater spread and ensuring an even quantity being distributed more effectively over a wider area. Petroleum is also used for weed-killers, soil conditioners, and for veterinary uses, where it has contributed largely to animal health. It is estimated that about 8 per cent of this country's consumption of oil products is used for agricultural purposes.

Lubrication

But petroleum's most important contribution to industry is the provision of probably 90 per cent of the whole of the lubricants, without which not only all the motor-cars, aeroplanes, ships and tractors driven by fuel oil, but also all the steam engines, electric trains and coal-fired vessels, would stop. Without oil to lubricate its machines, hardly a single industry, however small, could survive and the industrial world would cease to exist.

A hundred years ago, the only lubricants available were of animal or vegetable origin, and were called fixed or fatty oils, because they could not be distilled. These oils were excellent lubricants when used by the methods then adopted—by pouring on the bearings in what was called the 'all loss' manner, that is, once only through the bearings. Owing to their tendency to combine with oxygen and to undergo internal changes at higher temperatures, they could not be used under present-day conditions, which demand efficiency and economy. They have, however, the great advantage of adhering to metals and forming durable adsorbed layers. Vegetable oils have a drying tendency, which, particularly in the case of linseed oil, makes it valuable for making paints but bad for lubrication, and the animal oils become rancid, objectionable to handle and active on certain of the metals in general engineering use. Castor oil, rape oil and many other vegetable oils were available. Lard, sperm oil and neatsfoot oil were typical of the oils of animal origin which had proved valuable as lubricants.

The petroleum industry provides a source of mineral oils having very good lubricating properties, and the advantage over the fixed oils of being stable and resisting oxidisation both at normal and elevated temperatures. They are also chemically inert to all the metals used in engineering construction and can experience many cycles of heating and cooling without change of essential lubricating properties. They have the great advantage of being able to be blended and to have their characteristics adjusted in the manufacturing processes to provide oil suitable for lubricating the finest instruments or the heaviest engineering plant. The availability of improved lubricants has made it possible to design engine gears and many forms of machinery with heavier bearing loads, thus enabling space and weight to be saved and so gain great advantages in road, rail, marine and aircraft propelling machinery.

It was early discovered that 5 or 10 per cent of the fixed vegetable oils could be blended with mineral oil so as to increase the adsorption. Probably the best example was the combining of rape oil with mineral oil for lubrication of main-line steam locomotives. This has led to the development of a whole series of additives to mineral oils in order to provide the most suitable oil for any particular purpose. Additives have been used to develop oils with 'extreme pressure characteristics', which are called E.P. oils. These oils have enabled gears to work satisfactorily with greatly increased pressures between the teeth.

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The essential part that the oil industry has come to play during the past century in the life of an industrial nation such as Britain may be summarised as follows.

It provides petrol and diesel oil for more than 4,500,000 cars, vans and other vehicles for carriage of passengers and goods.

It provides fuel for every aeroplane, naval vessel, and the vast majority of the merchant fleet.

It provides fuel for the increased food production achieved in recent years.

It provides oil fuel for the steel, pottery, glass and ceramic industries and improves the quality and output of their products.

It helps the gas and electricity industries to increase their outputs.

It provides lubricating oil for 90 per cent of the world's requirements.

Oil is now helping industrial life by producing literally hundreds of petroleum derivatives used in processing and manufacturing products as diverse as insecticides, detergents, fibres, plastics, rubber, paint and artificial silks.

Conclusion

In the early part of this Address, I have referred to oil exploration, oil-well drilling, oil production, oil transportation, pipelines for refinery products, etc., and other phases of the oil industry, and you will appreciate how the services of engineering and almost every other profession, and of practically every industry, have been necessary in each of these phases in order to place the oil industry in the position it is today.

You will readily realise that all these services will be required in greater measure in the future, and just to take one example of particular interest to Sheffield, that of steel, it has recently been estimated that the oil industry of the free world, excluding the U.S.A., will require a total of 19 million tons during the next five years, 1956–60, in order to produce, refine and clothe in all its stages of movement from the wells, the oil the consumers will need. About half of this will be finished steels purchased by the industry itself and will fall into three main groups: piping of all kinds and well casing; tinplate and drumsheet for packaged oil products; and tankage and structures. The shipbuilding industry will require something like 5 million tons to fulfil the tanker orders. It is quite clear that the oil industry's demand for steel will increase substantially, and the rate of increase will probably be somewhere between 6 and 10 per cent. The steel industry is thus faced with the very serious problem of increasing its capacity if it is not to hold back the essential development of the oil industry.

Endless other similar examples may be taken to show the vast effort which must be made in order to enable the oil industry, not only to realise its natural growth, but to take over so much of the percentage now being filled by the coal and gas industries until nuclear power brings relief.

I wish to acknowledge the help which I have received from my former colleagues and from the Directors of several of the oil companies and to thank them for their assistance in preparing this Address.

BLOOD GROUPS AND HUMAN EVOLUTION[1]

ADDRESS BY DR. A. E. MOURANT

PRESIDENT OF SECTION H

Introduction

Eight years ago, at the Brighton meeting of the Association, a symposium on blood groups was held. Today we are holding another symposium dealing mainly with the same subject. In the interval the gains in knowledge have been immense, but perhaps equally important is the very great change which has taken place in attitude and outlook.

For many years now, anthropologists and medical research workers have accepted the theory of natural selection, but we in these fields of research have nevertheless tended to think of life, and especially of human populations, in terms of states rather than of processes. A given set of skull measurements or a given set of blood groups was regarded as defining an existing population; but we did not give sufficient thought to the process by which those characteristics arose. It was very convenient to be able to put individuals and communities into a number of pigeon-holes; but how much had we learned about them and about man in general when we had put them there? Incidentally, and of this I shall say more later, the blood-group pigeon-holes did not always correspond very closely with the skull pigeon-holes. Today biologists, including anthropologists, are thinking more and more in terms of change. That there have been changes is beyond doubt: it is certain that the present apparently stable populations with their various hereditary characteristics, their blood-group frequencies, their ranges of skeletal measurements, are descended from populations with other frequencies and other measurements. But the elucidation of the mechanism of those changes has scarcely begun—yet it has begun, and it is largely of those beginnings that I shall try to tell you.

What are Blood Groups?

Since, however, it is primarily of blood groups that I shall speak, it has been suggested to me that I ought first to explain what blood groups are and how they are inherited. It is well known that every human being belongs to one of four blood groups, A, B, O, and AB. This means that his or her red blood cells (or red corpuscles) carry on their surface one or the other of two substances known as A and B, or neither of them, or both. The 'blood group' is simply the symbol or symbols of the substances present, group O corresponding to the absence of both substances. The A and B substances themselves are complex polysaccharides, a class of natural products of which the best known members are starch and cellulose. The A and B substances can be distinguished from one another only by reactions with certain human or animal blood serum preparations. For instance, a serum containing something known as 'anti-A' will react with red cells carrying the A substance (i.e. of group A or AB) but will not affect B or O cells. Similarly, a serum containing anti-B will react only with B or AB cells. Blood-grouping is usually carried out by tests on red cells with each of these two types of serum. Reaction between cells and serum is shown by the cells clumping together, or agglutinating. In the absence of a reaction the cells remain uniformly suspended.

All the cells of one individual are of one blood group. This group, though not necessarily the same as that of either parent, has been inherited from them according to well-established rules, and will similarly be passed on to the children. I shall

[1] Address delivered at the Sheffield Meeting of the British Association on August 31, 1956.

try to explain this process, though in the time available I can give only a very rough outline of it.

The Genetics of the Blood Groups

Every body cell of every human being contains, in its nucleus, forty-eight thread-like bodies called chromosomes. These forty-eight consist of twenty-four pairs, the members of a given pair being similar but not identical.

The precise structure and organisation of a chromosome are matters upon which opinions differ, but for the moment we may perhaps be allowed to simplify matters and to compare it to a string of beads, each bead being known as a gene and affecting one simple body character, such as the blood group. Not only are the chromosomes as a whole paired, but the genes on them have a fixed linear order and are also paired in such a way that one gene on one chromosome and the gene in the corresponding position on the other chromosome together control a given character.

Each of the reproductive cells, however, contains only twenty-four chromosomes, one from each pair.

At fertilisation the twenty-four chromosomes from each parent come together, and the new complete set of forty-eight is reproduced in every cell of the embryo. Each of the two genes (one on each of a pair of chromosomes) which control the blood groups will belong to one of three possible varieties known as A, B and O. It will be seen that though, from the testing point of view, O is a negative character, representing the absence of A and B, there is nevertheless a separate O gene. The joint action of two A genes or of an A and an O makes a person belong to group A, of two B's or B and O, to group B; an A and a B gene determine group AB, and two O genes, group O. Each of the reproductive cells of the new individual will, in turn, carry either but not both of the genes received from the parents.

I have so far spoken only of the common or ABO blood groups, but there are now known to be at least nine separate systems of blood groups, each of which can be regarded for our present purpose as genetically independent of all the others and each of which is determined in the individual by a separate pair of genes. Though for any given system the individual possesses only two genes, there may, in the population as a whole, be two, three (e.g. A, B and O) or even more alternative varieties to which these two (which may be like or unlike) may belong (e.g. two A's, an A and a B, etc.).

For any genetical system, such as that of the ABO blood groups, it can be shown mathematically that, in a 'random mating population', the frequencies of the genes, and hence the frequencies of the different kinds of individual (i.e. of the blood groups), will tend to remain constant from generation to generation unless certain of the genes have a selective advantage over the others. We shall later consider the effects of natural selection, but it can be stated with confidence that for the blood groups the overall selective effects in a single generation are slight, and thus, at least over a small number of generations, the gene frequencies in a community may be regarded as constant quantities, characteristic of that community.

The Nature of the Genetical Data bearing on Anthropology

We have seen that there are at least nine blood-group systems. For each of these nine the frequencies of the various genes differ from one community to another in a manner which permits a classification of the communities.

The vast bulk of blood-group frequency data refers to the ABO system alone, and here we can classify to our heart's content, checked only by classical or morphological anthropology, and by occasional clues from history and archaeology. In general it may be said that where history shows populations to have a common origin they do have similar blood-group frequencies.

There exists, however, a large and growing volume of data on populations that

have been tested with regard to anything from two to all nine blood-group systems. We do not, as a rule, find that the classification deduced from one system agrees completely with that deduced from another. If they did agree, we should have available a very satisfactory classification of mankind; but it would be merely a static classification—a system of pigeon-holes—with little evidence as to how the existing state of affairs was reached. A consideration of apparent discrepancies has, however, always in science been a fruitful source of new knowledge, and may in the present case lead to an understanding of the processes whereby the present frequencies arose. Of these processes I shall speak later.

Ultimately, it is thought, all or nearly all bodily characters are controlled by the mechanism of chromosomes and genes already described, but it is only in a small proportion of cases that a group of alternative characters can be attributed to a single set of alternative or allelomorphic genes on a single pair of chromosomes. Moreover, in many cases the alternative characters are, on the one hand, 'normality' and, on the other hand, some severe but rare disease of little anthropological interest. For anthropological purposes we need two or more characters each reasonably common, and the most important known genetical systems of this kind are the blood-group systems.

There are, however, a number of other such systems, and perhaps the most important of these is that which affects the haemoglobin of the red cells. Practically all Europeans and a large majority of all human beings have a single kind of haemoglobin, haemoglobin A. In Africa between the tropics and sporadically in Southern Europe and Asia, we find that up to 20 or even 40 per cent of people have another type, haemoglobin S, usually mixed with A. In the absence of oxygen this haemoglobin crystallises inside the red cell, distorting it and rendering it liable to damage. This variety, and the others to be mentioned, are, however, most easily characterised by observing the speed of movement of a spot of haemoglobin solution on a strip of suitably moistened filter paper along which an electric current is passed, the process being known as paper electrophoresis. Owing to the different speeds a single spot containing a mixture of haemoglobins becomes separated into two (or more) spots.

A third variety of haemoglobin, C, reaches frequencies of about 10 per cent in West Africa, where it exists along with A and S. In South-east Asia, another variety, E, partly replaces A, as does D in Northern India. Other varieties of haemoglobin, known by the letters G, H, I, J, seem to be very rare. It appears highly probable that all these varieties, both the common and the rare ones, are the products of a single system of allelomorphic genes.

In the Mediterranean area, in Siam, and probably in parts of tropical Africa, a gene of another system gives rise to thalassaemia, or Mediterranean anaemia, by interference with haemoglobin production. Over large areas its frequency is high enough to make it of anthropological importance.

The abilities to taste phenylthiocarbamide and to smell hydrogen cyanide are each (separately) genetically controlled, but the tests are not as precise as could be desired, and the genetical situation is not entirely clear. If, however, these difficulties could be overcome, the frequencies of the characters concerned would certainly make them useful anthropological markers.

All the characters so far mentioned are inherited by relatively simple genetical mechanisms. This greatly facilitates the mathematical treatment and enables precise predictions to be made of the results of mixing of populations. Perhaps of equal importance is the fact that the blood groups, at least, are fixed genetical characters unaffected by state of nutrition and external influences generally.

With such characters we may contrast the external characters of the body, i.e. the size, shape and colour of the body and its parts. These are, in the main, inherited, but very little is known about the genetical mechanisms involved. The characters show continuous variation which can be described only in terms of some

kind of measurement, whether of colour or of size, as distinct from the sharp distinctions which may be made with regard to characters such as the blood groups. It is almost certain that each character (e.g. height, skin colour) is the resultant of the joint action of a large number of genes, so that it is doubtful whether a complete genetical analysis will ever be achieved. Darlington and Mather (1949) have, however, made great advances in the theoretical treatment of characters such as these, determined by what they call polygenes, and they have also admirably summarised existing knowledge of the subject.

Most of the external characters are, moreover, modified in the individual by external influences such as food supply and sunlight, and are not precisely determined expressions of the genes controlling them.

On the whole it may be said that the blood groups and certain other simple genetical characters are much more stable in the individual than the visible external characters, and that many, if not all of them, are perhaps more stable long-term features of the community than are the external characters.

Each of the main external characters is the product of a number of independent genes, presumably on several different chromosomes. For reasons readily explained statistically the frequency of each gene may fluctuate from one generation to the next, but, for a single character which is polygenic, that is to say, determined by the combined effects of numerous genes, it is most unlikely that all the gene frequencies will fluctuate together. Thus, over a small number of generations, especially in a small community, these statistical fluctuations will tend to affect the genetically simple blood groups more than the external characters with their polygenic inheritance.

There can be no doubt that the general picture of bodily shapes and sizes presented by a human community is gradually modified in the course of thousands of years as a result of natural selection; and that such selection has given rise to the close adaptation which the human physique shows, both generally and locally, towards its environment. It was at one time supposed that the blood groups were not appreciably subject to such influences, and hence were almost completely stable as indicators of ancestral relationships. It is now thought that, like external characters, blood-group frequencies change slowly through the millenia, but some of the blood groups may be more stable over long periods than most of the external characters.

While, however, selection operates upon such genes as are present at any moment in a community, it is a prerequisite that there should exist, for each chromosome locus concerned, two or more allelomorphic genes. The ultimate source of such genes is the process of mutation whereby a given gene changes into one of its allelomorphic forms. Such a change is a rare event which, under natural conditions, we can neither control nor predict. Most mutant genes appear to have harmful effects and are rapidly eliminated by selection, but when a mutation occurs which, in a particular environment, increases the chances of survival of its bearer, it will tend to spread through the population.

Having thus tried to set out the nature of the genetical data to be examined in relation to anthropology, I should like to turn to a consideration of more interesting and perhaps more speculative matters, such as the origin and development of the genetically controlled characters of man, and especially of those features which distinguish one community or race from another.

Genetics and Evolution

Bodily characters, whether they refer to bones or to blood groups, are, in the long run, as I have said, mainly the result of natural selection acting upon the genes available. When, however, Darwin put forward the concept of natural selection as the mechanism of evolution, he, like nearly every other scientist at the time, was completely unaware of the contemporary but obscurely published work of Mendel,

the founder of genetics, and it was half a century before this work became generally known and was confirmed. The initial application of genetics to the theory of evolution was almost exclusively the work of Sir Ronald Fisher (1930). He showed not only that natural selection was fully explicable in terms of genes, but that only if inheritance was by means of discrete entities, such as genes, could natural selection, as we know it, take place. I cannot even attempt to summarise his masterly exposition: to do so would demand several lectures. His work has, however, become so much a part of the biologist's way of thinking about evolution that it has coloured most subsequent work and is inevitably reflected in this address.

Let me now give a few examples of genetics applied to the study of man: my object here is not to present you with any cut-and-dried classification but to try to explore with you the ways in which a genetical and evolutionary approach can help us to see how man, and individual races of man, have become what they are, through the interaction of a number of internal and external factors. Perhaps we may even catch glimpses of future trends of human evolution. Certainly I hope that we shall see how a more extensive and thorough application of the principles which I shall illustrate may be expected to teach us much more than we now know about the species to which we all belong.

The Distribution of the ABO Blood Groups

Let us first look at the ABO blood groups. Thanks largely to the records of organised blood transfusion services, we know more about the distribution of the genes of the ABO system in man than we do about that of any other set of allelomorphic genes in man himself or in any species of animal or plant whatsoever (see Boyd, 1939; Mourant, 1954). I hope that in another year or two we shall have available a single compilation of all the published information on this subject, based on about four thousand separate investigations and giving data for several millions of people tested.

All the three genes of this system, A, B and O, are found in nearly all known populations, but their relative frequencies vary widely from one population to another. On these two slides (Mourant, 1954, maps 4 and 5) I have summarised most of the existing information on the distribution of the A and B genes. Each continent or major subdivision of a continent has its own general character. Nearly everywhere the O gene is the commonest of the three; in Europe, A is fairly common and B relatively rare. In Africa the preponderance of A over B is less. In Asia, B is very common and over large areas exceeds A in frequency—a tendency which spreads into Eastern Europe. Australia and America are peculiar in that B is almost completely absent from the main aboriginal populations and may a few hundred years ago have been entirely absent. In America the Eskimos in fact possess all three genes; the Indians of the United States and Canada are almost exclusively A and O; while from Mexico southward little but the O gene is found, and it is a tenable hypothesis that in the whole of what is now Latin America only the O gene was present before the white man arrived.

These generalisations, however (apart from those relating to the complete absence of one or two genes), refer only to the average values of the gene frequencies or, to use a geographical metaphor, they describe only the general levels for each region, but these are in fact diversified by numerous hills and valleys, for it is a peculiarity of the distribution of the genes of the ABO groups, as distinct from most other sets of genes, that even within quite small areas such as that of the British Isles they show marked frequency variations from one part to another.

There are, as I have already mentioned, at least eight other systems of blood groups, but only for two of these, the Rh and MNSs systems, have we anything like a complete world picture. In the case of the Rh system the gene-frequency differences between continents or other major regions are at least as great as for the

ABO system, but within a given area of continental size the local frequency differences are very much less than for ABO. The genes of the MNSs system likewise appear to be fairly uniformly distributed over large areas but the data are less full than for the Rh system.

This afternoon we are to hear accounts of the distribution of the blood groups in various parts of the world, which will, I hope, be regarded as bearing out what I have just said, and explicable in the way I am about to describe.

If we are thinking in terms of processes of differentiation between populations it appears to be a reasonable hypothesis that the major distinctions between the principal subdivisions of mankind took place many thousands of years ago, each subdivision having a characteristic set of frequencies for the genes of each of the blood-group systems. Subsequently, we may suppose that minor local differentiations arose between local races.

Natural selection by disease

In terms of this hypothesis, the frequencies of the genes governing the ABO system have been less stable in the last few thousand years than have those of the other two systems mentioned. Solely on the basis of gene frequency statistics, I had, nearly four years ago, come to such a conclusion; it is possible that other workers had reached a similar point of view. I felt that somehow the ABO genes were being affected by the processes of natural selection more rapidly than were those of the other blood-group systems, but the exact mechanisms involved were far from clear. It was soon after this that Aird, Bentall and Fraser Roberts (1953) first showed that the frequency of group A was significantly higher among sufferers from cancer of the stomach than among the general population. Subsequently it has been shown that group O is very much commoner in sufferers from duodenal ulcer than among healthy persons; and that there is an excess of group A among sufferers from diabetes. Other relations between blood groups and disease are now suspected to exist, and much work is in progress in this new field, which will be surveyed later this morning by Dr. Fraser Roberts.

There is no doubt that the diseases concerned are caused in part by the environment, and despite the late average age of onset of some of them, they do reduce the average number of children likely to be produced by the sufferers. Here, then, we appear to have a means whereby the environment is acting selectively, in the Darwinian sense, on the ABO blood-group genes. I do not wish to suggest that the diseases mentioned are themselves particularly important as agents of natural selection, but we can be almost certain that they are merely the first examples to be discovered of a host of conditions each producing its perhaps slight but cumulative effect. It is still very far from clear how the blood-group genes or their products, the blood-group substances on the surface of the red cells or in the tissues, can affect susceptibility to disease, but the clue may ultimately be supplied by work now in progress on the precise chemical nature of these substances and of the subtle distinctions between them.

It is also not clear why the ABO groups should be more markedly subject to such processes than the blood groups of other systems. This is perhaps connected in some way with the fact that the A and B substances, and antibodies reacting with them, are found in a great variety of animals and plants, giving rise to wide possible ramifications of selective action, whereas the substances connected with the MNSs and Rh systems are mainly confined to man and the primates.

THE RHESUS BLOOD GROUPS AND HAEMOLYTIC DISEASE OF THE NEWBORN

The case of haemolytic disease of the newborn might at first glance appear to be an exception to these generalisations for, as is well known, it arises from an incompatibility between the Rh blood groups of husband and wife, and leads to a selective

mortality of Rh-positive offspring. Until the discovery of transfusion therapy, however, the mortality seems to have borne little if any relation to the environment, and such selection as occurs is of a very different kind from that just considered, as a short description of the mechanism of the disease will show.

The genetics of the Rh system are most complex, but we need only consider for our present purpose a single pair of allelomorphic genes, D and d. The presence of one or two D genes in the genotype of the individual determines the presence of a substance or antigen D on the red cells, and such a person is said to be D-positive or Rh-positive. The homozygous dd individual lacks this substance and is said to be D-negative or Rh-negative. A woman who is of genotype dd will pass on one d gene to each of her children, but some or all of them may receive a D gene from their father, and they will thus have the D substance on their red cells. In certain cases, during pregnancy, this D substance crosses the placenta into the mother's circulation, where it stimulates certain of her tissues to produce an 'antibody', anti-D, which then circulates in her blood. The mechanism is similar to that whereby bacteria stimulate the production of antibodies which in turn damage or destroy the invading bacteria. The anti-D likewise, when it inevitably gets into the circulation of the foetus, damages and finally destroys many of its red blood cells, so that it is born suffering from haemolytic (or blood-destructive) disease of the newborn. The mortality of this disease, when untreated, is of the order of 30 per cent.

The children who die are all of genotype Dd, and thus, if we regard each individual in the community as represented by his or her pair of Rh genes (DD, Dd or dd), an equal number of each of the genes D and d is being destroyed by the disease. Thus in the long run, and in the absence of any possible compensatory process, whichever of the two genes is the rarer in the community as a whole will ultimately die out. This has interesting implications regarding the origin of the peoples of Europe, but we have not time to consider these now. The point which I wish to establish is that this particular process is one the ultimate effect of which is to produce not diversity but uniformity within a given interbreeding community and, having no direct relation to external environment, will not tend to produce local variations in frequency. It may be mentioned that other pairs of allelomorphic blood-group genes take part in a similar process and, acting as rare causes of haemolytic disease, must be subject to a similar though exceedingly slow levelling effect.

The Varieties of Haemoglobin

For the most clear-cut instance of interaction between genetical constitution and environment we must, however, turn from the blood groups to the haemoglobins.

As I have already said, most adults possess a single type of haemoglobin, haemoglobin A, but in tropical Africa many persons have a second type, haemoglobin S. The occurrence of these two haemoglobins is controlled by a pair of allelomorphic genes. Most people, even in Africa, are homozygous for the haemoglobin A gene (not to be confused with the blood group A gene) and possess only haemoglobin A, but about 15 per cent of the population of all tropical Africa are heterozygous and have a mixture of the two types. Heterozygotes of constitution AS almost certainly have, as we shall see, a rather greater chance of surviving to marry and have children than have normal AA homozygotes; but if we make the conservative assumption that they have equal chances we can calculate the probability of two parents each transmitting a gene for haemoglobin S to their child, and we deduce that about half of one per cent of all births must consist of homozygotes for haemoglobin S. Such homozygotes, apart from a small persistence of the foetal type of haemoglobin, have only haemoglobin S. As we have seen, the S variety of haemoglobin, when deprived of oxygen, becomes insoluble in the red-cell fluid and crystallises inside the cell, distorting its shape to something supposed to resemble a sickle.

This is liable to occur each time a cell, in the course of circulating in the blood, gives up its oxygen to the tissues, and the process is reversed when it is reoxygenated in the lungs. Thus the red cells rapidly become damaged and destroyed, a process known as haemolysis.

Hence homozygotes for haemoglobin S mostly suffer from a severe anaemia, known as sickle-cell anaemia. Most homozygotes die from this disease in early childhood, though probably a small but unknown proportion of them survive to adult life and have children.

Heterozygotes possess the two haemoglobins: there is usually more of A than of S, but we must, for the moment, resist the temptation to speculate as to why the amounts are not equal as we should expect. The two types can be separated from the natural mixture by the process of electrophoresis which I have already described, but when the mixture, inside the red cell or in free solution, is deprived of oxygen there is no separation but the mixture solidifies or crystallises as a whole. Its solubility is not, however, as low as that of pure haemoglobin S; the transformation of red cells to the sickle form can fairly readily be produced *in vitro* but probably rarely takes place in the body. This agrees with the observation that heterozygotes enjoy almost completely normal health and only under exceptional circumstances, such as during high flying in non-pressurised aircraft, are they subject to any disability referrable to their peculiarity.

The theoretical frequency of homozygous sicklers is, as we saw, about half per cent of the total births in tropical Africa, and reaches 4 per cent in some tribes. Many if not most of the homozygous babies will die without leaving offspring. If this elimination were not compensated in some way, the gene for haemoglobin S would be reduced to a low frequency in a few generations and would ultimately die out. In order to account for its present high and presumably persistent frequency, two main hypotheses have been considered.

One such hypothesis demands numerous mutations or changes from the gene for haemoglobin A to that for S in every generation. In order to compensate for the losses of haemoglobin S genes by the death of homozygotes it would be necessary for a proportion of the order of one per cent of haemoglobin A genes to mutate in each generation. Most mutation rates that have been measured, whether by indirect means in man or more directly in other organisms, are of the order of 1 in 100,000 per generation, and a rate of 1 in 100 is far outside the range of known rates, so that it cannot seriously be entertained.

The other theory is that heterozygous AS persons have more offspring than have those of either homozygous type, AA or SS. This situation is known as one of balanced polymorphism. It is clear that AS persons have more offspring than have those of genotype SS. To prove this hypothesis it would be necessary also to show that AS people had more children on the average than AA people. It is difficult to demonstrate such a situation directly, particularly in semi-civilised or uncivilised communities, but the hypothesis would receive very great support if it could be shown that AS people, in the conditions under which they live, were in some way healthier than AA people.

In the probably analogous but more confused case of thalassaemia, already mentioned, it was suggested by Haldane (1949) that heterozygotes for the thalassaemia gene were more resistant to malaria than homozygous normal people. In the case of haemoglobins A and S, the possibility that heterozygotes had a special resistance to malaria was considered by Raper (1950) and by Brain (1952); the fact of such resistance was first clearly demonstrated by Allison (1954) and subsequently confirmed by numerous workers, especially Raper (1956). There is still disagreement as to the magnitude of the effect, but nearly all workers agree that it exists; there can now be very little doubt that it is the true explanation of the persistence of sickling in Africa and elsewhere. It should be added that the increased resistance of

heterozygotes is mainly, if not entirely, to one type of malaria, the malignant tertian type, due to the parasite *Plasmodium falciparum*. The mechanism of resistance is not fully known but is probably by means of the better ability of the parasite to grow on haemoglobin A than on haemoglobin S.

With these clues to guide us, we are now able to realise that the frequency of the haemoglobin S gene in a given population must be due, at least in part, to the local incidence of malaria to which recent generations have been exposed. Thus we may suppose that, before the institution of effective anti-malarial measures, something like an equilibrium had been established for each population between the rate of malarial infection and haemoglobin gene frequencies. With the rapid elimination of malaria which is now taking place in many areas, the genes are no longer in equilibrium with the environment, and a process of evolution must be taking place, leading to a new genetical equilibrium. In some populations, as Allison (1956) has suggested, it may be possible to measure gene frequencies over a period of years or in successive generations and so directly to observe evolution taking place.

The Rates of Change of Anthropological Characteristics

We are here faced with a dilemma that will become more and more pronounced as anthropology more and more takes cognisance of genetical and evolutionary processes. We are, on the one hand, using gene frequencies as indices of relationship between populations and, on the other hand, seeing them being modified by the environment. Some genes, including probably most of the blood-group genes, are influenced only very slowly by the environment and are stable indicators of relationship. Others are more sensitive to the environment and so, while easier to investigate physiologically and medically, are less reliable as evidence of distant ancestry —but the differences are in every case differences in degree only; it is likely that every gene is ultimately subject to environmental control and that every gene is in some degree an indicator of relationship, even if over only a few generations. The study of the interplay between heredity and environment which together determine the frequency of genes and of characters is a growing field in which anthropology and genetics on the one hand and pathology and physiology on the other are mutually enriching one another.

Even when considered from the standpoint of pure classification, however, the varying rates of frequency change of different characters are in some ways an advantage. The stable characters enable us to classify mankind into broad major races within each of which all members may be presumed to share a common ancestry about ten thousand years ago. If, however, some characters had not responded to varying environments by undergoing selection within that period, we might be unable to make any subdivisions of the major races at all. Fortunately, we have the ABO blood groups which seem to respond to selection in periods of the order of two thousand years, and in some areas we have the haemoglobins which may so respond in only a few hundred years. Ultimately, when we know more about the present-day responses of genetical characters to the environment, we may be able to set up something like a hierarchy of probes of varying length into the past, the longer ones defining broad and stable divisions, the shorter ones smaller and less stable subdivisions of humanity.

The examination of human remains dug up by the archaeologist gives us much more direct information about individuals and races of the past. This information is, however, limited in two ways: firstly by the small numbers of bodies available, and secondly by the possible unreliability of the blood-grouping tests. Unless the bodies are highly fragmentary or distorted, linear measurements are comparable in accuracy with those made on the living, or on fresh corpses. Blood-group determinations, however, are not always possible, and when they are carried out the degree of accuracy, though it may be high, is always in some doubt since adequate control

tests cannot be performed: there is probably considerable scope for improvements in technique. Dr. Hodges will shortly tell us something of the present situation in blood-group archaeology.

I have so far spoken as though natural selection were the only process giving rise to gene frequency differences between populations. In small communities, however, random frequency fluctuations occur and may have a cumulative effect known as 'genetic drift'. The most important fluctuations are those that result in the total loss of a particular gene, when the few persons who happen to possess that gene die without passing it on to the next generation. There can hardly be any doubt that, in some very small communities that have been examined, the unusual or exceptional blood-group frequencies found are the results of such fluctuation. Since, however, a compact isolated community is also ideal for the operation of natural selection, it must not be too readily assumed that every anomalous blood-group picture found in a small community results solely from gene frequency fluctuation or drift.

So long as blood groups were taken to be neutral in relation to natural selection, genetic drift appeared to be the only possible source of the wide frequency variations observed. The vast interbreeding communities of recent historic times were, however, too large for any important random frequency fluctuations to occur. It was therefore assumed that the fluctuations had taken place before the advent of civilisation, when man was still a rare animal existing precariously in small communities. With increase in food supplies, numbers increased, and the accidental gene frequency distributions were perpetuated. It seems impossible with our present knowledge to decide how far it was such a process and how far it was natural selection that determined the broad continental frequencies of some genes that seem so thoroughly stabilised in the major races.

In the particular case of the American Indians the hypothesis of drift in a small community entering America from the north-west has been put forward with particular insistence, and it is indeed difficult to see how the characteristic gene frequencies of this race (for MN and Rh, for instance) could have arisen sufficiently rapidly simply by natural selection in the few hundreds or even thousands of years that these peoples may have spent in a uniform environment, and yet have subsequently persisted apparently unchanged over thousands of years to give rise to the remarkably uniform gene distribution which we now find in the greatest variety of environments from the Arctic through the tropics and on to the extreme south.

Let us now turn to the visible and measurable external characters of the body, its shape and colour, and those of its parts. These have been for untold millenia the diagnostic criteria of 'race', and they were until recently the only characters available to the physical anthropologist. The colours of the skin and hair are affected in the individual by exposure to sunlight; the size of the body is affected by nutrition; but these are, under normal circumstances, minor effects; the principal factor determining the characters is heredity. This was, in fact, realised long before genetics became a science.

Unlike the differences between the blood groups, nearly all the observed differences are of degree rather than of kind, and scientific accuracy demands precise measurement of the gradations. This is relatively easy in the case of differences of size; the stature of the whole body and the dimensions of its parts can be measured with simple instruments such as callipers. Colour differences are more difficult but not impossible of measurement. The basic data of physical anthropology consist mainly of measurements of length—millions of them. Great effort and ingenuity have been expended in the mathematical treatment of such data, so as to enable them to be used in comparing populations and estimating the degree of physical relationship between them. Since, however, little was known until recently about the mechanisms of inheritance of the continuously varying characters, little of this

mathematical treatment makes use of genetical concepts. It was indeed at one time supposed that these characters, whether in man or other organisms, were inherited other than by means of chromosomes and genes, the mechanism which had already been shown to account for the heredity of qualitative characters and differences. It has now been shown for many continuously varying characters, in a variety of animals and plants, that each such character (after making allowance for the effects of the environment) is the result of the combined effects of a number of genes, known as polygenes. There is no reason to suppose that man is exceptional in this respect, but so far little genetical work has been done on such characters in human beings. Sir Ronald Fisher (1918) carried out a pioneer study on some of the human characters, and Dr. Tanner (1953) has followed this up. This afternoon Dr. Healy will speak of a joint study of the problem as a whole, which he has made with Dr. Tanner.

Genetics as a Basis for Physical Anthropology

I do not, however, wish for a moment to give the impression that physical anthropologists are to blame for not taking cognisance sooner of genetics, or that, for lack of a genetical basis, their heroic efforts of measurement and correlation are of little value. The more primitive peoples of the world are still covered far more completely and usefully by the records of physical anthropologists than by those of blood-groupers, and if the genetical point of view is to gain ground the onus is on geneticists to apply their specialised methods to the study of human morphology and pigmentation. The task will not be an easy one, but its results are likely to add greatly to our understanding of the physical relationships existing between human populations. Perhaps we may look forward to two ways in which, among others, the knowledge gained may be put to use. It may show which measurements have the simplest genetical basis and hence are likely to be the most profitable to make. It may also increase the yield of useful information to be derived from the accumulated measurements of the past. But even if the direct study of the genetics of these characters does not prove to be as fruitful as one now hopes, the genetical standpoint is nevertheless likely to help greatly in the study of the adaptive and evolutionary aspects of the characters concerned, and to act as a unifying principle between the studies of the genetically simple characters such as the blood groups, and the polygenic morphological characters.

I wish to stress that there can be no real incompatibility between the information supplied by blood groups and that yielded by morphological characters. Each kind of evidence must add to the information yielded by the other. From time to time, however, instances are discovered of apparent discrepancies between racial classifications based on blood groups on the one hand and on morphological characters on the other. Sometimes, too, the evidence of one blood-group system appears to be at variance with that of another such system. In Europe, for instance, ABO blood-group zones run approximately at right angles to those for the Rh groups.

Granted that in a given case the observations are technically accurate and the differences statistically significant, it will usually not be difficult to suggest a mechanism for the origin of the differences, based on natural selection and the other established agents of evolution, though it may be very difficult to prove any one hypothesis. It might, for instance, happen that a pair of slowly selected alternative characters showed a north–south gradient due to recent progressive mixing between two populations. Upon this might be superimposed an east–west gradient due to the effect of climatic or other geographical zones upon a pair of rapidly selected characters.

Blood groups in animals

I have hitherto today spoken of blood groups as though they were a phenomenon confined to human beings; but we can be sure that many of the human

blood groups are derived from pre-human primates, and we do in fact find the ABO system almost completely developed in anthropoid apes, and can recognise in them forerunners of the MN and Rh systems; moreover, something akin to the ABO system is present in many species of mammals.

Though it is partly because of more intensive research that the human blood groups appear more complex than those of most other animals, the numerous comparative investigations seem to show a true increase of complexity in man as compared with the higher apes, and probably in the latter as compared with monkeys. Most species of mammals tested have, however, been shown each to possess several systems of blood groups, and in the case of cattle the total picture is extremely complex and specialised, clearly as a result of independent evolution.

When we contemplate the blood groups of animals it is useful for us, as anthropologists, to remember not only man's animal ancestry but also his ecological relation to the animal world, and the fact that for thousands of years human tribes have wandered the world accompanied by flocks and herds of domestic animals from which are descended the present breeds. Blood-group frequency differences between breeds are found to be comparable to those between human races. Later Dr. Evans is going to speak to us of these differences and of their possible relation to human migrations.

We have now looked at a great many ways in which genetics, and especially the genetics of the blood groups, impinges upon anthropology. The picture is a rich, varied, intensely interesting one, but if we are to gain more than an aesthetic satisfaction from it we must realise that, with all its complexities, it is a closely integrated one in which every part is dependent upon every other.

A Summary of Human History

In order to emphasise that interdependence and unity, I shall try in the next few minutes to summarise, in terms of genetics, the whole history of mankind.

There is little doubt that the final steps in brain development, from advanced ape to primitive man, took place somewhere in Africa or Southern Asia. The rate of evolution of the brain and of the containing skull may have been very rapid indeed without necessarily being accompanied by any marked change in the other parts of the body, or in such characters as the blood groups, all, we may suppose, well adapted to local conditions.

Then the ability to communicate by speech and to use tools enabled small groups of families migrating from the primitive surroundings to dominate progressively a wide variety of different environments and ultimately to colonise almost the whole world.

To a large extent the environments themselves were adapted, as by the building of houses and the wearing of clothing, to suit the newcomers, but some features of the habitat could not be adapted; and only in so far as he could adapt himself by evolution could man survive in them. Thus there came about the development of a great variety of local physical types. And more subtle biochemical and physiological characters, among them the blood groups, had also to undergo selection and in many cases to arise by mutation and spread by selection. We have hardly begun to understand how it is that particular blood groups are favoured by particular environments, but, as we have seen, it is already known that a number of diseases, related to such features of the environment as diet and infection, affect preferentially persons of certain blood groups, and so serve as agents of selection. It may thus be that the wide variations in gene frequency within blood-group systems, and the large number of the genetically separate blood-group systems possessed by man, are an essential part of the response by what was originally a local tropical species of primate to the very varied climatic and ecological conditions with which it was faced.

Such a genetical flowering, analogous in many ways with the birth of a host of reptilian species in the Triassic period, and of mammalian species in the Eocene, probably took place in a number of stages mostly corresponding to the ends of the successive glacial episodes. The process is continuing, but is for the moment masked by the great mixing of recent centuries. It was, however, almost certainly after the end of the last glaciation, the Würmian, that the major subdivisions of the human race, as we know them today, were established. To this period we must attribute the laying down, mainly through selection but perhaps in part by random gene frequency fluctuations in small tribal groups, of the broad genetical constitution of these races, a constitution expressed on the one hand in their well-known physical characters and on the other in the more stable parts of their blood-group pictures.

In the relatively short succeeding period, migration and mixing have carried populations and genes to and fro over the earth, but selection has had a marked effect upon relatively few characters; among these we must almost certainly include the ABO blood groups, and quite certainly some of the haemoglobin variants.

Despite its slow operation we cannot doubt that evolution is continuing. We may hope even to be able to observe it and measure its rate as, for instance, in producing a diminution in frequency of the gene for sickle-cell haemoglobin in a malaria-free environment. This too is an example of how civilisation, by removing some of the cruder selective agencies, may have an effect which can truly be described as eugenic.

A study of such processes in action here and now would be in keeping with the present spirit of anthropological research, and a manifestation of the development, to which I think we can look forward, of a unified physical anthropology firmly based on the whole of human biology. Such an anthropology will take into account all the anatomical and physiological characters of man, tracing, where possible, their development by the methods of palaeontology and embryology, and studying their relationship at the present time both to heredity and to environment. Above all, it will include an examination of the interplay between the last two factors, to the mutual enrichment of physical anthropology and of all the other biological sciences of man.

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MACHINERY OF POSTURE[1]

ADDRESS BY PROF. D. WHITTERIDGE, F.R.S.

PRESIDENT OF SECTION I

My text is taken from *The Testament of Beauty*, where Bridges remarks in the opening lines:

> 'Our stability is but balance, and conduct lies
> in masterful administration of the unforeseen.'

My theme, however, is simpler; not conduct, but merely balance, those devices which maintain our posture without conscious thought on our part, and which react quickly, smoothly and accurately to the unforeseen. An acceptable account of this machinery in terms of postural reflexes was produced by Rudolf Magnus just over thirty years ago. He was inspired by Sherrington's analysis of reflex behaviour of the spinal cord, and built up an account of the interaction of reflexes to which very little has been subsequently added. The picture drawn by Sherrington describes perfectly adequately the postural machinery of the decerebrate cat in which the whole of the forebrain has been removed. The basic mechanism is the stretch reflex of each antigravity muscle supplemented by the more widespread effect of the stretch of the interosseus muscles of the feet. With these mechanisms the leg can become a rigid pillar able to increase the force of contractions of its muscles if further weight is put on them. The decerebrate cat, in fact, stands if put on its feet. It is not, however, stable, and if it begins to sway, the variations in stretch reflexes are not sufficient by themselves to prevent falling.

Magnus drew attention to the reflexes arising from the vestibular apparatus and neck in these animals. If a decerebrate cat's head is turned as though to look up at a shelf, after a latency of several seconds the forelimbs extend and the hind limbs flex. Clearly here the stretch reflexes of the hind limb have been inhibited by the tonic reflexes from the labyrinth. To this extent we can follow Sherrington in regarding posture as the product of proprioceptive and labyrinthine reactions which appear to reinforce mutually. Where they clash, the labyrinthine reactions predominate.

It is noteworthy that Sherrington repeatedly mentions the compensatory action of stretch reflexes, that they permit a muscle to maintain a constant tension in spite of considerable variations in its length, that posture becomes plastic in a decerebrate cat, in the sense that a variety of minor differences, left hind limb in front of right hind limb and vice versa, are compatible with steady weight-bearing.

Sherrington emphasises the steady persistence of standing in the decerebrate cat as compared with the chronic spinal cat or dog in which the stretch reflexes of the hind limbs are strong enough to take the weight of the animal, but the legs are liable to flex suddenly for little reason. The decerebrate cat may continue to stand 'like a statue' for hours if no outside forces push it off its balance.

However, what Sherrington calls compensatory actions, and we might perhaps call stabilising reactions, have very limited scope in the decerebrate cat.

If we move the head, there are no rapid reactions of the trunk or limbs. The reactions already described have latencies of many seconds. As far as I know, the only rapid compensatory reaction which can be elicited from a decerebrate cat is contraction of the eye muscles during head movement of such a direction as to tend to keep the visual axes fixed in space. These reactions have latencies measured in milliseconds and can be set up in all eye muscles by appropriate movement.

[1] Address delivered at the Sheffield Meeting of the British Association on September 3, 1956.

In these decerebrate animals the optic nerves are cut and they are unquestionably blind. Nevertheless, the remarks of Sherrington clearly apply to the way in which their movement is controlled. 'The retinae—those refined photoreceptive patches—are set movably in the head. By the action of muscles they can retain their bearing to the horizon although the head itself shifts its bearing to the horizon. These compensatory reflexes as well as the habitual maintenance of the primary harmonious attitude are largely traceable to the operation of reflex arcs which arise in the receptors of the labyrinth.'

There are then two aspects of these reflexes; firstly, their simple existence, secondly, if, and only if, their action is exactly related to movement of the head, the visual axes may be stabilised in space.

It has perhaps been too readily assumed that the mere existence of a reflex will ensure smooth and steady execution of a movement. A simple example is the pilot who is given a course on which to fly, with a radio signal which tells him whether he is on course, or off it, to the left or to the right. If, when he is off course, he flies a course which makes an angle of 30° with the required course, the note in his headphones will change as he crosses the track he should be on, and he must overshoot for a time depending on his reaction time, the manoeuvrability of the plane, and his speed. His course will therefore be a series of zigzags about the course required. If we wish him to get on a course without oscillation, he must be provided with a signal which tells him at any moment, not only that he is off course to the left, say, but roughly how far he is off the track prescribed. As he approaches he can change course so as to reduce his correcting angle progressively.

In the same way the mere presence of a reflex will not guarantee the performance of a movement smoothly and without overshoot; the movement of the limb must be controlled by signals set up by its position, and by its rate of movement, which must then be related to the required end of the movement.

When we turn to the preparation most used by Magnus, the mid-brain animal, i.e. one deprived of its cortex and incapable of sensation, we find it has righting reflexes whereby if placed in any position it will turn its head until it is horizontal and follow by righting its trunk and limbs. These righting reflexes as regards the external world are initiated by the labyrinth as regards the angle between head and neck by the proprioceptors from the vertebral joints and the neck muscles. Pressure on one side of the trunk will make the animal right itself in the absence of neck and labyrinthine mechanisms.

There are, however, a few points in the work of Magnus which should be noticed. Nearly all the observations have been made on the slow or final reactions of the body to displacement, no doubt because visual observation or slow-speed recording methods were then usual. The reactions studied were relatively gross, the presence or absence of the ability of the animal to right itself. It is as though we were to study the means by which we struggle to our feet again after a fall while learning to skate, but omit to study the small but important movements of our centre of gravity while we are precariously perched on our skates.

The only case of compensatory reflexes in Sherrington's sense dealt previously with in detail is the stabilisation of the rabbit's eye. With nodding movements of the head about a bitemporal axis, compensatory rolling movements are required about the visual axis, since the eyes in the rabbit look out approximately 90° from the mid-line. This rolling movement is so extensive that the head can be moved through 100° without any change in the position of the eye relative to the horizon. Magnus and de Kleijn comment on the efficiency of this mechanism for keeping the image of the horizon fixed on the retina.

When we turn to posture in the monkey we find some interesting observations by Bieber and Fulton. They found that removal of the whole of the motor and premotor cortex on both sides abolished 'voluntary' movement and that vestibular

postures then appeared. They concluded that vestibular reflexes were normally inhibited by the motor areas. This idea is now widespread and is incapable of proof or disproof for the great part of the body musculature which is under voluntary control. The muscles which rotate the eye are not under voluntary control, but they are clearly subject to control by the vestibular apparatus. A possible view is that postural reactions can be reviewed by the cerebral mechanisms and inhibited or utilised and built into a more elaborate scheme of movement, just as the vestibular mechanisms can utilise or inhibit the stretch reflex.

This would be consistent with the findings of Bard on hopping reactions. These consist of a displacement of the trunk which is followed by a hopping movement of the limb to a position in which it can more effectively support the trunk. These reactions depend on the integrity of the motor cortex. When this area is removed, labyrinthine reactions though present are weakened.

There are also many observations on the results of labyrinthectomy, from which the monkey recovers more quickly than the cat or dog, and the conclusion has been drawn that the labyrinth is of little importance in the higher animals. I hope to show that this is false.

There is a vast literature on the effects, in man, of stimulating the vestibular apparatus by rotation or by local warming and cooling, but there is comparatively little on the maintenance of the erect posture. This is perhaps one of the few fields in which electromyography has provided new and interesting data. Kelton and Wright point out that the muscles of the trunk and legs are inactive for long periods during 'easy standing', but that swaying in the fore and aft plane produces contraction of tibialis or of soleus. The amplitude of swaying movement between the onset of contraction of one muscle and the other corresponds to a movement of about 24′ at the ankle joint. They conclude that this is not a stretch reflex since the ankle has to be moved some 2–5° while the subject is lying down in order to elicit action potentials. However, they show that the movement required decreases with increasing load on the part, and presumably one would need to load the foot with a weight equivalent to that of the body to make a fair comparison. For the moment, therefore, we can draw no conclusions about the interrelation of stretch reflexes and labyrinthine reactions in man. The most interesting developments, however, have been made in the study of bilateral labyrinthine destruction in man. Although it has been said in the past that the resulting deficits can be completely compensated in man by visual righting reactions and by proprioceptive reflexes, this is not the case. Sudden passive movement usually leads to overbalancing since visual reaction time is too long, and purely proprioceptive reactions may not be entirely appropriate. There is a considerable French literature on the behaviour of patients on tilting tables from observations by André-Thomas, Rademaker and Garcin, and by Zador, a literature which is largely ignored by Anglo-American writers. When patients who have bilateral destruction of the labyrinth are examined standing, sitting, lying, or on all fours on a tilting table, they show no compensatory reactions whatever to sudden tilt. A normal subject reacts without any training in such a way as to keep his body over his centre of gravity. It is a remarkable fact that patients with tabes with complete loss of proprioceptive mechanisms, and patients with cerebellar lesions, both show compensatory reactions to tilt, though the speed and extent of their reactions may not be normal.

Similarly, sudden passive movement disturbs visual function in patients who have had both vestibular nerves cut. This was carried out in the middle thirties by Dandy for bilateral Ménière's disease. After the initial period of vertigo had settled down, the patients complained that although vision was normal while they were at rest, when walking or being wheeled over rough ground their vision was 'jumbled' so badly that they could not recognise faces. Spiegel and Sommer, who reported these cases in a text-book of neurology, describe this as a 'surprising finding', words

used also by Dandy. To us, perhaps only their surprise is at all odd. If the head is being made to nod about a bitemporal axis, we know that normally there are reflex contractions of the superior and inferior recti by which the visual axis can be kept horizontal. We can guess that it is necessary to keep the image of a friend's face steady upon the retina, so that each feature may be examined by the fovea and the eye moved so that the eyes, nose and mouth fall in turn on the fovea as the most sensitive area. If the stabilising mechanisms were to fail, we should no more expect to recognise a face than we should hope to recognise a microscope slide of a tissue which was being jerked about at random under the high-power objective. This explanation of the cause of the disabilities of these patients was given by the American neurologist F. B. Walsh.

A simple mechanical analogy perhaps makes the situation clearer. After the war Commander Gairdner, discussing servo-mechanisms used in the Navy, pointed out the importance of stabilising the platform which carried a telescope for observing enemy aircraft. He remarks, 'It is not possible to track a small target continuously from a platform which is rolling erratically and unpredictably'. It was therefore necessary to stabilise the platform against roll and against change of course, so that the axis of the telescope would remain constant relative to the horizon. As the enemy aircraft could then be kept in the visual field, the observer was able to keep it on cross-wires by manual controls on the telescope. His movements and the adjustments put on to the platform from stable elements and the gyro-compass could be passed to a computer, and the product used to control automatically the anti-aircraft guns.

It is clear that to some extent the head is used to provide a stable platform for the eyes. This is the only mechanism present in the smaller birds which do not move their eyes. Their heads move in jerks, but between whiles are held quite still while the image falling on the fovea is examined. Obviously the eyes may be directed to any point, but during fixation, the head is stabilised against external movement.

An extreme example of this skill is the ability of hawks to hover so still that they may be seen to remain on the cross-wires of a telescope even during a fluctuating 30-m.p.h. wind. Presumably this stability is necessary for the bird to quarter the ground below for some sign of movement by its prey. In man the ability to compensate for forced movement of the body or head is considerable. It is possible to read a newspaper on top of the fast-disappearing Edinburgh trams, which produce violent head-nodding of a period of 1 in 2–3 seconds. This is only possible by vertical compensatory movements of the eyes of considerable amplitude, and in some people the effort required produces fatigue and perhaps nausea. Similar repeated vertical movements of the head and eyes may play a part in the fatigue of long-distance car driving. Irregular passive movement is also responsible for difficulties in map-reading in a moving car. Most people then find it impossible to read the smallest print of an Ordance Survey map. The advantage of stabilisation of the eyeball is best seen when we attempt to pick up and follow a very small target. If our eyes are forced to move, the image of the target will move out into the peripheral field where it may be below the visual threshold, and long and laborious scanning movements will be needed to pick it up again. If the eye is approximately stabilised we may hope that at worst the image may be displaced to the para foveal area where it can still be perceived and returned to the fovea by the fixation reflex. The advantage of stabilisation, then, is that it is prerequisite for the operation of the fixation reflex, which can be superimposed on the stabilisation mechanism.

We are now in a position to weigh the relative merits of describing events in the central nervous system in terms of reflexes or of servo-mechanisms. Already a certain amount of abuse can be heard. For Fulton, cybernetics is 'a jejune term which explains nothing, offers nothing conceptually new, and has spread unhappy confusion among students, especially those of an engineering and mathematical

bent. "Feedback" thinking is of course the basis of Sherrington's proprioceptive system'. Sir Francis Walshe doubts if the term 'cybernetics' will be an enduring influence. I am not here concerned with the wider aspects of cybernetics, but am solely concerned with the applicability to the nervous system of ideas derived from the study of servo-mechanisms.

Perhaps we should say that in recent years we have oversimplified our ideas of reflexes by studying them under conditions so artificial that their meaning is lost; that the simple reflex which Sherrington called a useful if not a probable hypothesis is becoming ever more complex; a reflex may be present and yet lead to undesired or irregular movement, tremor or nystagmus if its force, onset and duration are not precisely controlled. This is self-evident, and clearly was frequently in Sherrington's mind, but its importance was far from being sufficiently emphasised in classical neurophysiology. That a neurosurgeon and neurologists could fail to see the significance of 'jumbling of vision' in a patient with bilateral labyrinthectomy seems to me sufficient evidence that the concept of stabilisation was not previously in current use. Finally, the stage at which the stability or instability of a biological system can be predicted from a knowledge of the properties of its servo loops is one which is in sight and represents a new development.

The simplest example of a servo-mechanism is of course the steam-engine governor, in which, as every schoolboy knows, a pair of heavy balls revolve about a shaft. If the rate of revolution of the shaft increases, the balls move further out from the shaft and, in doing so, decrease the supply of steam to the cylinders. This is 'negative feedback' whereby a change in the system is automatically opposed. To a biologist the governor resembles the single cells in *Hydra* which detect the presence of an enemy and themselves fling a dart into it. The governor has a 'sensory' system (the balls) directly coupled to a 'motor' system (the steam valve). However, during evolution the animal develops separate sensory and motor cells with nerves and a nervous system between. The machine develops signalling systems which can transmit continuously information from sensory devices, though this only occurred for mechanical devices a few years before the last war. In a developed servo-system we have a detector device or amplifier which may also differentiate or integrate the signals fed in, and an effector device which reduces the size of the original signal. In the same way the great majority of reflexes consists of reactions of an animal to a stimulus by which that stimulus is removed. A cook drops the hot saucepan she has picked up, pupil contraction cuts down the amount of light falling on the retina. To this extent 'reflexes' and servo-mechanisms are equivalent. In the last twenty years, it has become so usual to study reflexes by means of artificial stimuli that their significance tends to be lost. If we evoke a flexor reflex by a single electric shock lasting about one thousandth of a second, the shock is over long before any movement can take place. If we pull on a tendon, the muscle contracts and opposes the pull. If, however, we tap the tendon, the tap is over long before the muscle 'jerk' begins. These artificial situations are so useful for the study of central processes in reflex activity—for that intensively cultivated field of synaptology—that when we emerge with two and three neuron arcs we are surprised to find that these processes seldom occur in nature, and never in a pure form.

The stretch reflex exemplifies the increasing complexity of even a simple reflex. Basically the primary or nuclear bag ending of the muscle spindle responds to stretch and excites the motoneurone. However, stretch also excites the tendon organs which inhibit the same motoneurones. We know now that the efferent fibres to the intrafusal muscle fibres of the muscle spindle, the γ motoneurones, can profoundly modify the sensitivity of the nuclear bag receptor to stretch. These γ motoneurones are played upon by a variety of higher centres as well as afferent fibres in the spinal cord. It is therefore doubtful whether it is useful to think of the

stretch reflex isolated by section of the cord as a basic unit, rather than as a mutilated part of a larger unit.

The same consideration also applies in transferring ideas from lower mammals to primates and man. It is likely that, with increasing encephalisation, the amount of activity which can be carried out by the isolated cord decreases in the primates. This point was stressed by Sherrington, though with our more recent knowledge of the capabilities of the human isolated spinal cord when it is not damaged by toxic absorption from bed sores and urinary infection, it has been overlooked. It remains true, however, that the human spinal cord can offer no activity as elaborate and as precisely co-ordinated as the scratch reflex of the spinal dog. Now we know that γ efferents can be modified by higher centres, there seems little point in debating whether the checking of swaying movements in man is carried out by stretch or labyrinthine reflexes. Labyrinthine reflexes will act by modifying the stretch reflexes through the γ efferents, and the excitability of stretch reflexes depends on labyrinthine activity. Perhaps one may say that in the higher animals the Jacksonian lowest level of integration has become less capable of independent activity.

The conditions for stability of a thermostat, level observation platform and the erect posture in man are much the same. In all cases we must have a sensory device capable of detecting small deviations from the resting position. This must operate with as short a latency as possible. The effector mechanism must operate with sufficient power and add as little inertia as possible, though this may be an irreducible feature of the system. The two most important features are sensitivity and speed of action. Clearly in the past we have been content with observing the gross presence or absence of reflexes. The minimum stimulus needed to excite, the minimum latent period, the prevention of overswing have not been studied in relation to stable posture. The relation of the cerebellum to the vestibular apparatus illustrates this most clearly. Before 1914, the cerebellum was called by Sherrington the head ganglion of the proprioceptive system and was generally believed to be closely concerned with balance. In 1921, however, appeared the work of Magnus and de Kleijn in which they state that righting reflexes persist after total removal of the cerebellum. Careful reading of their work, however, shows that their animals which survived cerebellectomy had indeed righting reactions, but these reactions were far from being perfectly executed. In their protocols the extent of rotation of the head which could be compensated by rolling movements of the eyes was grossly decreased. There seems little doubt that the cerebellum is in fact essential for the precise performance of righting reactions. According to Rademaker and Garcin, patients with cerebellar lesions react normally to sudden movement on the tilt-table. However, according to Zador their performance is far from normal. In some of his patients their reaction to tilt was so exaggerated that they fell over backwards.

The investigation of the effects of cerebellar lesions on the latency, intensity, and duration of vestibular reactions has scarcely begun. We now know that such investigations will have to take into account the differences between cortical lesions and those involving the roof nuclei, and also differences between lesions of the fastigial, intermediate and dentate nuclei. This may, however, be a rewarding approach.

It is well known from the work of Gordon Holmes during the 1914–18 war that the latency of voluntary movements is considerably increased on the side of a cerebellar lesion. His direct measurements put this beyond doubt. If the arm is supported and suddenly allowed to fall, it is well known that the arm on the affected side falls further before it can be checked by the subject, and it usually oscillates for a little while before it is brought to rest. On the other hand, though the knee-jerk is said to become pendular, there is, as one would expect, no change

in its latent period. There are as yet apparently no measurements on the effect of different lesions of the cerebellum on the latency of labyrinthine movements. We know that these reactions can still occur in the absence of the cerebellum. However, we also know that the reticular formation plays an important part, particularly in the ocular labyrinthine reactions, and we know the cerebellum is closely related to the reticular formation.

The history of our knowledge of the labyrinthine reactions which produce rotation of the eyes about the visual axis in man is a curious and interesting one. De Kleijn established that rabbits can compensate for head movement of 100° about a bitemporal axis. According to the present-day standard text-books, in both man and the pigeon, compensation is much less complete, the eyes only move through one-tenth or less of the angular movement of the head. However, it was known to Breuer and to Mulder in 1875 that this small angular movement in man was merely a residual compensation, that during the side-to-side rotation of the head there was a much bigger movement which decayed in 1–2 seconds after the end of the head movement. This considerable degree of compensation during actual head movement was not very clearly described, and apparently was thought to be of so little importance that it disappeared from the text-books. Quite recently Dr. P. A. Merton has reinvestigated the question and shown that compensation is in fact complete during movement of up to 30°, and that visual acuity is not decreased during head movement. Dr. Sommer and I have made similar observations on eye movement in the pigeon, prompted by Merton's work. During movement of not more than 10°, compensation is perfect, but the eye moves back afterwards either by a slow drift or by a quick flick, particularly during the next blink. Presumably during flight or while swinging from branch to branch it is essential to be able to fixate the next landing point.

I think, therefore, there is a good case for saying that although the concept of stabilising or compensatory reactions in nerve physiology has a respectable history, the essential requirements for stabilisation have received very little attention. If we concentrate our attention on the conditions which affect the latency, force and duration of vestibular reactions, we may reasonably expect some new insight into the machinery of stable posture. The key to the unravelling of postural deficits has been analysis by sudden passive movement—the reaction to the unforeseen. When we know how we react quickly and precisely we shall understand something more of the wisdom of the body, the masterful administration of the unforeseen.

PSYCHOLOGY AND THE HUMANISTS[1]

ADDRESS BY PROFESSOR JAMES DREVER

PRESIDENT OF SECTION J

WHEN I was younger and much in the company of philosophers I used to hear it said that 'You can't have a science of psychology'. In the words of Wittgenstein, at whose feet I once sat entranced and bewildered, 'The existence of experimental methods makes us think we have the means of solving the problems which trouble us, though problems and methods pass one another by.' The philosophers in those days were talking about an introspective psychology, and the present pre-eminence of the behavioural approach suggests that they may have been right. They may not like the behavioural approach any better, but at least they seem prepared nowadays to grant that it is possible. Quite recently, however, this too has been called in question, and one hears it said, this time by educators, moralists, and reflective laymen, that 'You can't have a science of behaviour'.

No doubt we can blame in part the contemporary climate of opinion, a depression centred over Hiroshima. We are very sensitive now to the neutrality of science, its diabolic as well as its angelic potentialities. More specifically in the behavioural field there have been many things to cause the humanists discomfort. Reasonable misgivings may easily be produced by the grading of schoolchildren like eggs or potatoes, the polling of political opinion in advance of political decision, the ethical relativity which grows so naturally out of anthropological studies, and so on. But however well justified they may seem in detail, I am sure that the humanists are wrong in principle, and I should like to try and say why. Many of you will feel that my arguments are obvious, and they certainly are not new. At best I hope they may be timely.

Let us begin by spending a few minutes on the topic of science in general. People often think of a science as a collection of facts: chemical facts, botanical facts, geological facts, and so on. This is misleading. There is an early exploratory stage in the development of most sciences when such a description may seem to apply, but here it would be more correct to speak about the collection of data. In any case, as soon as a science begins to make progress, it does so, not by discovering new facts or data, but by inventing new ways of talking or thinking about what it already has. Before we go on to consider how it tries to do this let us look at some of the other, non-scientific, ways of talking that we use.

First of all we can narrate or tell a story either about natural events or about people and their actions. A story may be true or false, but these are not the only categories we can use. Many stories do not pretend to be true; they are meant to be interesting or to point a moral, and if they succeed in their purpose that is all that is asked of them. Moreover, the word 'true' is used ambiguously. We can say of a story that it is 'true to life' or that it embodies a 'profound truth about human nature' without insisting upon 'literal truth' in the sense that the events of the story must have happened. But even a literally true story about a real person, his biography in other words, is not science, though it may provide insights and hypotheses for scientific research.

The same holds good of description. It may provide a starting point, but it is not in itself necessarily scientific. It can be true in the sense of accurate or vivid, and the contents of an accurate description of somebody are undoubtedly facts about him; but scientific statements require to be more than merely true or factual ones.

[1] Address delivered at the Sheffield Meeting of the British Association on Thursday, August 30, 1956.

'He is a tall thin man', or even 'He is 74 inches high and weighs 150 pounds', might help the police under certain circumstances but they do not form part of the science of physical anthropology.

Probably 'systematic' is the best single word to identify scientific statements, though it is not by itself sufficient since philosophical statements for example are also systematic. This means that from one statement in a science it should be possible to reach others by the inferences and implications which it is the function of scientific theory to provide. In the less-advanced sciences these inferences are mostly of a very simple kind, like deducing the consequences of systems of classification or empirical generalisations: e.g. Mammals are warm-blooded. This creature is a mammal. Therefore this creature must be warm-blooded. Where theories are devised for which a mathematical model is available, as for example in physics, the systematic structure becomes at once highly abstract and extremely close-knit and the drawing of inferences correspondingly far-reaching and easy. The first kind of science is often called descriptive and the second explanatory, though some sciences are mixed, with explanatory theories in limited areas but not over the field as a whole. Even a descriptive science, however, is not just a description. Its generalisations and classifications take us at least one step in systematic abstractness above the level of ordinary conversation. For example, 'There is a big orange beast with black vertical stripes' is clear enough by way of description, and 'There is a tiger' is even better; but 'There is a member of the order Carnivora the family Felidae and the species *tigris*' places what has been observed in a wide scheme of zoological classification with various morphological and familial implications. Pompous though it may seem, this last statement says more, because it brings this scheme to bear upon the situation with which it deals. In the advanced sciences the implications of any statement are much wider and the network of relationships in which it is embedded more complex.

One rather common misunderstanding may occur at this stage. It is the view that the ordinary man is really trying to say '*Felis tigris*' instead of 'big striped beast' or just plain 'tiger', but, being ignorant and non-scientific, he is unable to do so. In other words, ordinary conversation is an inferior version of scientific communication, and common sense is rather muddled science. Probably this view arises from the fact that many of those who have studied the functions of language have been logicians, and have naturally been most concerned with its informative and systematic aspects. But any first-hand study of language behaviour, of people talking to one another, shows how limited in their relevance some of the logician's categories really are. Perhaps the view might be plausible if our non-scientific ways of talking were confined to narration or description. Science might seem to be doing the same job but doing it more economically and systematically. But, to stick to our example, what about 'Tyger, Tyger burning bright'? It is plain nonsense to say that this is just an emotional and inaccurate way of saying '*Felis tigris*'. The same holds good, though less strikingly, for much of our ordinary conversation. Not a few of our current perplexities spring from a tendency to assume that science is doing something better when it is really doing something different.

Now let us return to the point from which we started: to the statement that 'You can't have a science of behaviour'. This is a statement which can mean two different kinds of thing and it is often used in a muddled way. 'You can't smoke in the dining-room' is different from 'You can't smoke (without equipment) at the bottom of the swimming-pool'. One is a prohibition and the other a statement of fact. Going back to the claim that 'You can't have a science of behaviour', there is often to be found an underlying prohibition which gives voice to attitudes like 'How dare you treat people as things', or 'Human beings must be free'. These spring from a reasonable misunderstanding of the situation, which will be discussed presently. At the same time it should be clear that nobody is entitled to prohibit

scientific study however much he may dislike it. If we take the claim that a science of behaviour is impossible to be one of fact, then we require evidence. The evidence usually presented is the lack of success so apparent in the behavioural sciences today: but this is evidence of difficulty not of impossibility. In any case the claim is merely a screen for the real basis of the argument, which is 'You can't have a science of me'. The ordinary man tends to cling obstinately to this opinion, except perhaps when his misery may drive him to a psychiatrist for help. He forgets that in zoology and physiology, or even physics and chemistry, sciences of him already exist. The zoologist regards man as a species of mammal related to the apes, the chemist may think of him as composed of hydrocarbons and water with various trace elements. It is true that neither the chemist nor the zoologist maintains his scientific description when talking to his neighbour over the garden fence, but this does not mean that the description is inappropriate, a whimsy thought up in the laboratory and invalid outside it. My neighbour is *homo sapiens*, he is mostly H_2O, and he is my neighbour. This is the basis of the 'reasonable misunderstanding' referred to above, and must be dealt with before we go any further.

There is one general consideration which may at the outset do something to allay the anxieties of the reluctant humanist. This is that the psychologist, in stating his theories, is not entitled to leave himself out. Any generalisation he may make about behaviour refers to his own behaviour just as much as to anyone else's. If he claims that we are nothing but bundles of conditioned reflexes, then his science is included just as much as your morals, and it is nonsense, unless we are out-and-out pragmatists, to claim that a particular set of reflexes is true. It is the phrase 'nothing but' that constitutes the error here. A scientist may quite well offer to give an account of behaviour using only the concept of conditioning if he feels that it is wide enough for his purposes. That is to say, he may put forward a simplified pattern as something which will relate different kinds of behaviour to one another, and introduce thereby some sort of order into the bewildering variety of events in this field. This is his job as a scientist, and one cannot criticise him for doing it. One can only complain that his pattern does not fit, or that there are better alternatives. If, on the other hand, he says that behaviour is nothing but conditioned reflexes, whatever that would mean, he is not being a scientist at all, but a rather naïve metaphysician, who has committed himself to a position which, taken seriously, would make both metaphysics and science nonsensical.

There are those who feel that an argument of this kind that has just been sketched out is merely 'tricky'. Like Berkeley's demonstration that existing is equivalent to being perceived, it admits no refutation but produces no conviction. So the problems may be approached from another angle. One of the reasons why some people feel that a scientific study of behaviour represents a threat to their own integrity or to the society in which they live is that such a study seems to regard behaviour as causally determined, and this in turn is thought to deprive men of responsibility for their actions. This belief, whether consciously held or not, may express itself, for instance, in hostility towards the psychiatrist giving evidence in a criminal trial as well as towards the psychologist in his scientific investigations. Actually there are two problems mixed up here, though it is by no means easy to disentangle them. Let us take first the activities of a purely scientific kind. One of the most widely held beliefs about science is that it is a search for causes, an attempt to find out what makes what happen. But this is not the primary task of science. The search for causes is a diagnostic enterprise carried on by the practical man, the doctor, for example, or the engineer, rather than the physiologist or physicist. When we talk about the cause of anything we generally mean some particular item in its history without which it would have turned out differently. More especially it is an item we can try to control or change, since every event has a multitude of causal ancestors. Someone has a cold in the head, we are told, because he has a virus not because

he has a head. The reliable identifications of this kind of item is clearly quite essential whenever we attempt to increase our mastery over our environments. Science, by clarifying the structure of events, makes such identification much easier, but science itself need not talk about causes at all. What the scientist, does as soon as he passes beyond the descriptive level, is to construct theories or models which seem to fit the regularities observed in the world round about him. He then goes on to experiment so that he can determine how widely they apply.

To begin with, of course, he spends a good deal of time trying to discover what regularities there are, since until he has found some he cannot even begin to theorise. But all he need assume is that there does exist some lawfulness in his data which would make his undertaking worthwhile.

Now to say that human behaviour is lawful or intelligible does not imply a particular kind of lawfulness, one appropriates, for instance, to a billiard table, where the balls are moved hither and thither by forces outside themselves. In other words, the scientific study of human behaviour involves in the first instance no more than the belief that such behaviour, for all its complex variability, makes some kind of sense.

But, it may be objected, what is lawful is predictable and what is predictable is determined. Even granting the validity of what is at best a very dubious generalisation, the question of predictability is in this context almost certainly a red herring. Its irrelevance shows up as soon as we get down to cases. Suppose you take two men, one a good citizen, stable, law-abiding and well disposed, the other an emotional psychopath: which of them is likely to be more predictable? And on the other hand, which of them is likely to be more at the mercy of impulse or caprice, less free in the moralist's sense?

Taking the analysis a stage further we find that the predictions of the physicist are not as a rule made about nature in the raw, but only about nature in the laboratory. He has his models and blueprints, and he wants to know whether they apply in some particular area. If they do apply, then he should be able to make something by following the directions, it may be a green flash or a needle deflection, or anything else so long as he specifies it in advance. Once he has made the green flash, he can make it again, for his blueprints and directions are all explicit and he now knows that they work. In the same way an industrialist can predict that a tractor and not a bicycle will come off the end of his production line. But give the physicist a little bit of nature in the raw, an irregularly-shaped stone perhaps, and tell him to roll it down a hillside predicting in advance where it will come to rest. Theoretically it might be done, but an army of physicists would have to work for months on the job. What they were doing would not be physics either. To predict the consequences of a theory and then verify them is a very different operation from clairvoyance or prophecy.

In any case, when we say that a man is free and responsible we are not making a scientific or indeed a factual statement about him. We are rather giving notice that we propose to treat him in a certain way. He is responsible, we say in effect, unless he is mad or very stupid or somehow coerced by forces beyond control. He is to be praised, blamed, respected, or persuaded as may seem appropriate. We do not have to prove this: we are in fact stating some of the conditions necessary before the activity of proving something can have meaning. The difficulties arise only when we try to define situations in which we should not treat people in this way. Low-grade mental defectives, we feel, are unable to function in a fully human way, so we look after them, train them in simple skills, humour them, and pity them. The insane likewise are not held accountable for what they do or say. But every now and again a borderline case arises. A crime, let us say, is committed by a high-grade defective or emotionally unbalanced person and we have to decide what to do

about it. Here the only questions of fact concern the kind and degree of the offender's abnormality. Whether or not in the end he is held responsible expresses a decision on the part of society, a decision which should take the psychiatric evidence into account but is in no sense a summary of that evidence or another way of saying the same thing.

More than anything else it is necessary to keep in mind the complexity of our relationships with one another and the impossibility of reducing them all to some single type. For instance, if I say to my wife 'Please pass the salt', I do not think of my request as the auditory stimulus most likely to elicit salt-passing behaviour in the bourgeois Scottish housewife. If I did she would be entitled to throw the salt at my head, for my request would fall in the same category as 'Fetch it, Rover'. But a social psychologist might describe what I had said in that way, and prove his point by trying other formulations like 'Salt here', or 'Hey you, I want the salt', and showing that they did not produce the desired response as frequently. In the same way a student of child development might claim that my saying 'Please' was related to the fact that I had been consistently rewarded for so doing while I was being taught table-manners. But the essential thing to realise here is that in a social context my request is a request and not a stimulus or a habit. In other contexts, however, we may regard it in other ways. Trouble will only arise if the scientists claim that their ways define what the request *really* is, as though the various possibilities were in some sense mutually exclusive.

Even where we are not participants but are only trying to describe or explain, there is still no single way in which we have to go about it. Take another bit of human behaviour, this time one that has been embodied in a physical object such as a painting. From one point of view a painting consists of metallic oxides in linseed oil smeared on a flat surface. Thus chemistry supplies some relevant information and can be used, for example, to detect forgeries or provide stable pigments. But clearly, to make a chemical analysis of a painting square inch by square inch would be a laborious and trivial task and would leave many questions unanswered, however well it might be carried out. Applying the paint to the canvas was a physical operation and could presumably be rendered in terms of distance, direction, leverage, muscle-contraction and so on; but Socrates long ago pointed out how inadequate was this kind of account of human behaviour, inadequate but not false. Suppose the painting to be a portrait or a landscape, then one might profitably consider how well and by what means it represented its subject-matter. It might be interesting to try and find out why the painter chose the subject-matter he did, but even to this one question no single kind of answer need be sought. Reference to a passing mood might satisfy some; others might go back to the painter's childhood, his relationship with his mother, who were his friends and teachers; others again might look to social history, the growth of trade in seventeenth-century Holland for example, and the demand for certain kinds of wall decoration. The geometry of the painting might be studied and the laws which seem to underlie a satisfying composition of colour, mass, perspective. Even the reactions of a casual visitor to the gallery in which it had been hung might contribute something. Clearly there is no one answer that is complete and exclusive in fact or in principle. We try to make sense of our experience in various ways, and many of these are quite independent of one another. The chemistry of bleaching is not related to the family life of painters, nor the mechanics of a brush moving across canvas to the economics of patronage for the arts.

Several points seem to emerge from all this. One is that the 'reasonable misunderstanding' from which the discussion started is a misunderstanding. Those who feel that the scientific study of human behaviour constitutes some sort of threat have not stayed long enough to examine the grounds for their anxiety. The scientist attempts to make certain kinds of sense from his data but does not deny

the possibility of other kinds of sense. On the contrary, his whole activity as a scientist involves the assumption that other kinds of sense exist.

In the second place, there is a tendency for people to become confused over the part played by prediction in science and the extent to which it depends upon laboratory specifications and controls. Forecasting has always been possible, and knowledge helps to make it more reliable. A knowledge of form reduces the odds against us on the racecourse, and meteorology is better than twinges in helping us to predict the weather; but statements about the winner of the 3.30 or the wind strength over Dogger Back fall outside the scope of science, not because they are unreliable, but because they are not the sort of statements which it is the business of science to make. So with human behaviour: the clinician may make a prognosis, and his scientific knowledge will help him, but his prognosis remains the same sort of reasonable guess about the future that we are all making all the time.

Thirdly, the study of people as things, whether as electrons, or hydrocarbons, or organisms, can only take place in a social and historical context which involves a great many other relationships between the scientist and his subject-matter. The fact that some of these relationships may be ignored for particular purposes does not mean that they are denied or superseded.

Fourthly, it is important that those who are looking at psychology from the outside should realise the diversity of the activities included under the term. When it is said that you can't have a science of behaviour, what in fact is being ruled out? We all take it for granted, layman and psychologist alike, that behaviour is not a random chaotic sort of thing: its regularities can be identified and described. Aesop thought so, as did Plutarch and Theophrastus, so the notion is not something new-fangled and therefore suspect. From this point it is possible to move in many directions. Regularities of behaviour can be related to regularities at the physiological level or to genetic factors: what the subject does can be related to what he says about his inner experience or to things that have happened to him in his early life: certain kinds of measurement are possible instead of verbal description, and so on. Finally, explanatory models of various kinds and in various regions can be tried out and developed to the limits of their applicability. This last step presents enormous difficulties and has only been reached here and there, but the natural history of *homo sapiens* has been in the making for many centuries. All that Wundt and Freud and Pavlov did was to bring some partly new techniques to bear on old problems.

Our humanists then, and I do not suggest for a moment that all humanists are of this persuasion, have no sort of a case when they argue against a scientific study of human behaviour, always provided that such a study does not make claims which it cannot substantiate. They may seem to be in a stronger position when they complain about the uses to which psychological knowledge has been put. Here the trouble seems to be that they cannot accept at its face value the neutrality of science. 'He who is not for us is against us', seems to be their motto, and they misdirect their complaints accordingly. A sucker is born every minute, and our science may enable us to exploit him more efficiently. On the other hand, it could be used to identify and educate him. For certain educational, military and industrial purposes it is useful to classify people and deal with them according to their common characteristics. On the other hand, when we do this we are apt to lose sight of individual variations. Clearly we are now in a region where we have to make choices, to decide whether *A* or *B* is the better course of action. We are in exactly the same position when we decide what to do with atomic energy. Our science may help us in assessing the alternatives and their probable consequences, but it will not do our choosing for us. If you dislike the skilful exploitations of human weaknesses in advertising and propaganda, then say so to business men and politicians. If you feel that streamlining schoolchildren in terms of their academic aptitude is not a good idea, then tell the educational administrators. They may not agree with you, and may even be

able to convince you that they are right, but the scientist does not come into the picture except in the role of expert witness.

It follows from all this that a society which contains psychologists does not differ in any fundamental way from one which contains only physicists and biologists. In fact, psychology is only a particular specialism within biology. The society with psychologists is, however, more advantageously placed in that it can get rather more information on which to base some of its decisions.

Perhaps I have been pummelling my man of straw for long enough now. I should like to spend the rest of my time trying to set him alight, though not with any destructive intent. Let us assume that I have proved my case, and that our troubles do belong more in the area of decision-making than that of science. In drawing this distinction I should make it clear that I am not saying there can be no science of decision-making, but simply that having a science of decision-making is not the same thing as making decisions. Now in the past when we have had to make important decisions, as in morals, politics, or education, there have always been able and reflective men who were prepared to offer us guidance, either by clarifying the issues or evaluating the ends which we might seek to realise. We called these men saints or philosophers and sometimes followed their advice. Occasionally a philosopher might also be a scientist, Descartes for example, or even Aristotle; but he was not a philosopher because he was a scientist. He just happened to feel he should reflect rather broadly on some of our more basic problems. Few people today seem to have this inclination. Indeed, philosophy has tended to become a specialism among other specialisms, and many philosophers seem to be men who cultivate a particularly tidy way of talking about ways of talking.

It is this more than anything else which probably underlies the humanist's complaint, not the presumption of science but the abdication of philosophy. For ordinary men as well as educators and politicians have to go on making decisions, and they look to somebody for guidance. The humanist points resolutely back into the past: Plato may help here, Rousseau there. Everything worth saying has already been said, and the aim of education is to teach the young where they may find it. Rightly or wrongly, however, a man making contemporary decisions wants contemporary guidance, and that is where scientists are apt to be forced into a position where they do not belong. It has been found that the scientist can do rather well in dealing with means and consequences; perhaps he can give advice about ends as well. But in this field the scientist has no special credentials. He is probably an intelligent and well-intentioned fellow, but the humanist may be right to resent the undue consideration which is sometimes given to what his scientific colleague has to say on general matters.

It would seem that the remedy lies with the humanist himself. Instead of a timid antiquarianism, he should attempt to recapture the broad insight and speculative boldness of his predecessors. He might not measure up to Plato, but at least he could do better than Monsieur Poujade or Joe McCarthy.

To this the humanist may reasonably reply that it is all very well for me to talk, but how does one acquire insight and speculative boldness? He has a point there. Some contemporary research of Guilford's might provide a few hints, but I doubt it. The task is harder now than it has ever been, but also more important. In so far as our humanist is a reflective man concerned with ends and possibilities, it is his job, and his predecessors did not do it for him. If he has something worthwhile to say, ordinary men will listen, but some of the others may be a trial. He must have some sort of answer when the professional philosopher buttonholes him in the street and asks, 'Just what do you mean when you say that *X* is better than *Y*?' But the hardest cross of all to bear with equanimity may be the psychologist's remark, at once legitimate and irrelevant, 'Look! There goes a humanist. I wonder how he got that way.'

QUATERNARY HISTORY AND THE BRITISH FLORA[1]

ADDRESS BY DR. H. GODWIN, F.R.S.

PRESIDENT OF SECTION K

A PARTICULAR scientific interest has always been attached to the history of the British flora: it has in some sense, indeed, stood as a test-piece in the science of biogeography. Both the flora and fauna of the British Isles have probably been more exhaustively studied than those of any comparably sized area in the world, and our knowledge of the distribution patterns is detailed and extensive though admittedly far from complete. Some species are seen to be strongly southern or eastern in range, some are restricted to mountains and northern latitudes, and some have areas which are disjunct whilst others exhibit a continuous distribution.

The aim of biogeographic study is of course to elucidate the *mechanisms* by which these patterns have been produced and maintained. In substantial part this is a matter of control by climatic or edaphic factors acting upon the tolerances of the species concerned, but it is generally conceded nevertheless that the problems of distributional pattern are also inevitably *historical*. That this is so we may see from a quotation from the distinguished American biogeographer, E. S. Deevey, who writes: 'One of the most popular topics in all biogeography is the question whether the whole of the fauna and flora of the British Isles immigrated in Post-glacial times, or whether some fraction survived from an earlier time.' This is but one of many similar historical issues which biogeographers in this country must acknowledge and investigate.

For my own part I subscribe to the view put by Wulff: 'Biogeographical conclusions only find definite confirmation when it is possible to base them on a geological foundation, which historical geology can alone provide.'

For reasons which will become apparent, the period of geological history requisite for study of biogeography in this country is the Quaternary period, an epoch which lasted over perhaps half a million years, and which embraced the time of the so-called 'Great Ice Age'. It is now generally thought that within this period there were several main periods of glaciation (possibly four), with warmer interglacial periods between, in which vegetation prevailed like that which now occupies temperate Europe.

We may indeed for the purposes of this lecture go further and restrict our historical survey to the time since the end of the last Interglacial period, that is to say the Weichsel glaciation (with some interstadial recessions), the Late-glacial period (covering the Allerød warm period with the colder periods just preceding and following it), and the Post-glacial period (showing progress to a climatic optimum and recession therefrom). A schematic representation of these periods is given in Fig. 1.

The maximum extension of the Weichsel glaciation in this country is shown by the line of the New Drift which includes the York moraine in the east and crosses south-central Ireland from just north of Wexford. Stages in the retreat of the ice northwards are shown by moraine concentrations at the Scottish Readvance stage, the Stranraer-Lammermuir stage, the Perth Readvance and the Highland glaciation. The researches of Dr. J. J. Donner indicate that the last of these corresponds with the cold spell following the Allerød warm period.

The very recent development of radio-carbon dating has allowed us to give the absolute datings shown in our figure. This method, conceived and established by

[1] Address delivered to Section K (Botany) at the Sheffield Meeting of the British Association on Thursday, August 30, 1956.

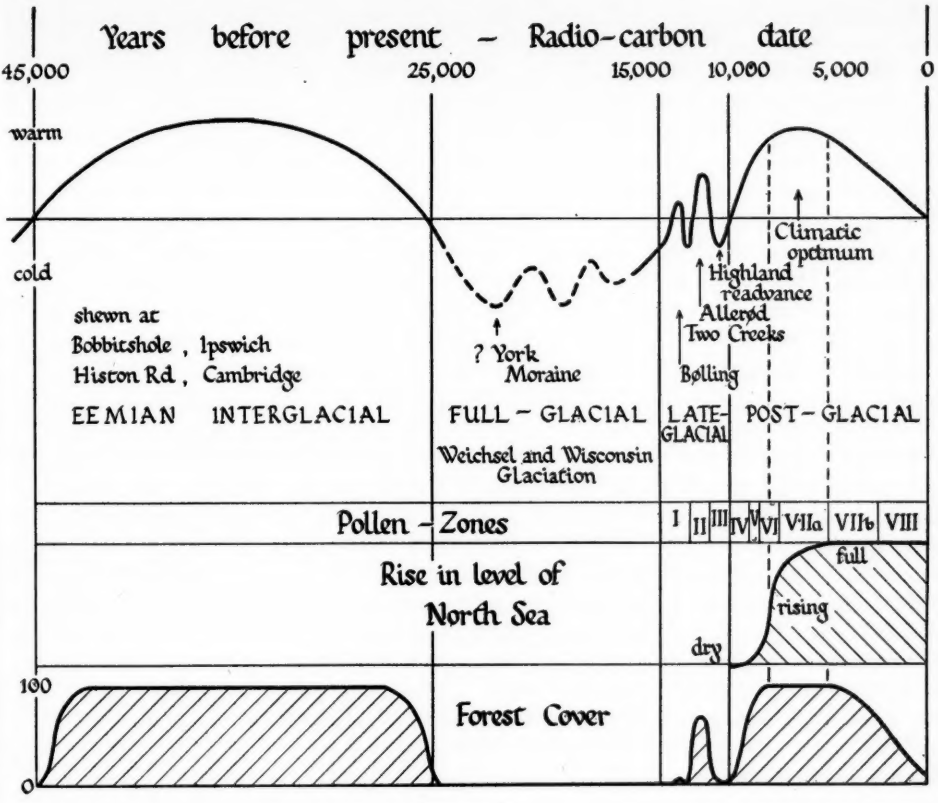

Figure 1

Professor W. F. Libby of Chicago, depends upon the cosmic ray flux in the outer atmosphere producing neutrons which, upon collision with nitrogen atoms, convert them to radioactive carbon, the C^{14}, which becomes oxidised to carbon dioxide and forms part of the great reservoir of this gas in the atmosphere and oceans of the world. Since the production rate is held to be constant and the decay rate is invariable (at about 5,570 years half-life) the total amount of C^{14} and its concentration in the air remain constant. Thus each green plant assimilating carbon dioxide builds into the starch, cellulose and, indeed, all organic compounds a constant although extremely small fraction of radio-carbon. There are great technical difficulties in measuring the very low intensities of radio-carbon found in organic material because the background of cosmic radiation and the natural radiation of walls, floors, apparatus-materials and even human bodies are relatively large. When these difficulties have been overcome it is found that 1 litre of carbon dioxide formed from burning contemporary wood will register about six or seven disintegrations per minute. Once formed, the organic compounds do not exchange carbon with the air, and thus a sample of wood 5,570 years old will be found to have an activity of half that of the contemporary sample. Thus the method, with suitable precautions, provides a basis for absolute datings, and these have in fact been successfully extended back for some 50,000 years.

An extremely important method of Quaternary study is also provided by pollen-analysis or palynology, the quantitative study of air-borne spores and pollen-grains which become embedded in very great frequency in lake-muds and peat bogs. By suitable techniques they can be concentrated and mounted on slides for microscopic examination. They retain such abundant and clear morphological characters that they can be identified and the proportions of the different kinds estimated.

Deposits from lakes and bogs thus reveal the former vegetational composition of the countryside, and series of samples through them present so consistent a picture of the sequence of climatic and vegetational events that these can be co-ordinated to form a series of pollen-zones applicable to the whole country and invaluable as a key to the general background of the history of plants, animals and man throughout the Late-glacial and Post-glacial periods. This method makes it evident that in England during the warm oscillation of the Late-glacial (Zone II) birch woodland prevailed, whilst in the cold periods before and after conditions allowed only open herbaceous vegetation with dwarf shrubs such as arctic willow and dwarf birch. After Zone III dense birch forest returned, and then was itself replaced in part by pine associated with dense hazel thickets. Now the spread of the warmth-demanding mixed-oak-forest trees and the alder displaced both pine and birch, and such woodlands remained dense and intact until Neolithic time. Thenceforward pollen-analyses also record the progressive disforestation of the countryside by man's adoption and extension of plant- and animal-husbandry. Likewise it records in southern and eastern England the very late expansion of the beech and hornbeam and the general return of birch to something of its earlier importance.

We have now two great assets in approaching biogeographical problems: first, a good general knowledge of the prevalent conditions of the Late Quaternary times, and secondly, very numerous fossil plant records from deposits which may be allocated, by one or more of the means indicated, to definite stages in this late part of the Quaternary period. On the basis of such knowledge we may now reconsider one or two of the familiar problems of plant geography.

Let us first consider the so-called 'glacial refugia'. In certain well-known mountain areas such as Ben Lawers, Ben Bulben, Teesdale and Cwm Idwal there are collections of arctic-alpine plants hardly found at all, collectively or individually in the intervening country. It has been strongly urged by many that this restriction is due to the fact that these areas were nunataks free from ice in the Last Glaciation and that the arctic-alpine plants there survived, and have not since been able to extend their ranges. Similar hypotheses have been strongly put forward to cover similar geographical phenomena in North America by Fernald, and in Scandinavia by Nordhagen. We need not now consider the extension of similar explanations to other distribution patterns, such as those of the Hiberno-Lusitanian flora, nor the various *ecological* arguments brought against this type of explanation. It suffices our present purpose to consider maps showing discoveries of characteristic members of these disjunct mountain assemblages in the British Isles during the Late-glacial period, that is to say *after* glaciation had here wasted to an end. If in this way we consider *Thalictrum alpinum*, *Dryas octopetala*, *Salix herbacea*, *Betula nana* and *Polemonium coeruleum*, we find that in each case the plant was widespread in the lowlands of this country. It therefore follows that any explanation of these restricted and disjunct northern ranges must be in terms, not of the glaciation itself, but of the Post-glacial period. At one stroke we are forced to abandon the theory of per-glacial survival *in situ* for these arctic-alpine aggregates: so far as these famous mountain localities *are* refugia, they must indeed be refugia not from the ice, but from the later conditions of the Post-glacial period. We may hazard indeed that in these mountain habitats they must have found escape from the dense forest cover that then submerged our landscape, from the peat bogs that formed on all flat or gently sloping mountain country, and from the warmer climate itself of the low-lands. We have indeed pollen-analytic evidence that in Cwm Idwal some of the Late-glacial plants which now belong to the high mountain zone were there during the height of the Post-glacial forest growth as our hypothesis demands.

It will be noted that *Betula nana*, recorded fossil from the Late-glacial in Ireland, no longer grows there. In this instance we prove Post-glacial extinction, and Mitchell has shown similar evidence exists also for *Minuartia stricta*, *Luzula spicata*

and *Astragalus alpinus*. Almost more striking is the case of *Ephedra distachya*, a gymnosperm now not found growing nearer than the French Biscay coast. Yet its pollen grains have been recovered from Late-glacial deposits in Scotland and the Lea Valley (North London) among other places in North-west Europe. We may not now simply explain the present-day absence of *Ephedra* from Britain by its supposed failure to have reached our coasts. The remarkable discovery in the last few years of *Koenigia islandica* growing in Skye, is accompanied now by a record of the unmistakeable pollen of this plant from deposits of Late-glacial age in Berwickshire, and it seems highly probable that this species too has diminished its range since Late-glacial time. We should here pause to note that certain European high arctic and high alpine species were found in the deposits of the Full-glacial period itself. These, which include *Papaver alpinum*, *Arenaria biflora*, *Potentilla nivalis*, *Festuca halleri*, *Gentiana cruciata*, *Potentilla nivea*, *Ranunculus hyperboreus* and *Pedicularis hirsuta*, are no longer found in the British Isles, and may have become extinct before the Late-glacial period.

A second problem of phytogeographical importance is the status of our weeds and ruderal plants. These have often been dismissed as the camp-followers of agriculture, introductions by Neolithic and later races along with crop seed, stock, ballast and litter. The evidence of sub-fossil plant remains goes back to situations far beyond the earliest human records of plant or animal husbandry. Even in the Great Interglacial we have records of such plants as *Heracleum sphondylium*, *Pastinaca sativa*, and species of *Valerianella*, and many members of the *Chenopodiaceae*. During the Late-glacial period, however, with its fresh unleached soils and freedom from tree competition, the 'weed' flora was extremely abundant, as is testified to both by pollen and by macroscopic remains. The genera *Artemisia*, *Rumex* and *Galium* have high pollen frequencies, and we recognise such plants also as *Centaurea cyanus* (cornflower), *Linaria vulgaris* (toadflax), *Pastinaca sativa* (parsnip), together with species of *Sonchus*, *Carduus*, *Taraxacum* and *Galeopsis*. It seems certain that these plants persisted through the ensuing phase of dense forest cover in specialised habitats such as river-banks, coastal cliffs and dunes, and sub-alpine zones of mountains reaching above the tree-line. With the coming of agriculture these species undoubtedly returned in strength to the lowland areas from which forest competition had excluded them, and they may well have been augmented by fresh ecotypes and new species brought from overseas by man.

It is not to be supposed that the period of the Climatic Optimum exhibited only *restriction* of plant ranges. Just as the Late-glacial offered optimal chance for migration and multiplication of plants requiring open habitats and tolerant of some degree of cold, so the Climatic Optimum was the period offering similar advantages to woodland plants and those requiring high temperatures. Surprising numbers of thermophilous species have indeed already been proved present in Late-glacial times; but others, such as the ivy (*Hedera helix*), holly (*Ilex aquifolium*), linden (*Tilia cordata*) and ash (*Fraxinus excelsior*), did not expand until the Post-glacial period was well advanced. Some of them indeed, such as *Najas marina*, reached a maximal extension at the Climatic Optimum and have subsequently become much reduced in range, not only here but on the Continent.

By turning to West European records we can easily multiply such instances or add to them elegant examples of the same effect in animals, as witness the evidence for the European pond-tortoise (*Emys orbicularis*).

We might very schematically represent the histories of the categories we have mentioned in the form of Fig. 2. It is necessary to add that there remain other categories of plants with wide ranges of tolerance, and wide geographical range, which have less pronounced variations in frequency than those indicated. Such curves, schematic as they are, are not just guesses at the past, they depend directly upon substantial records.

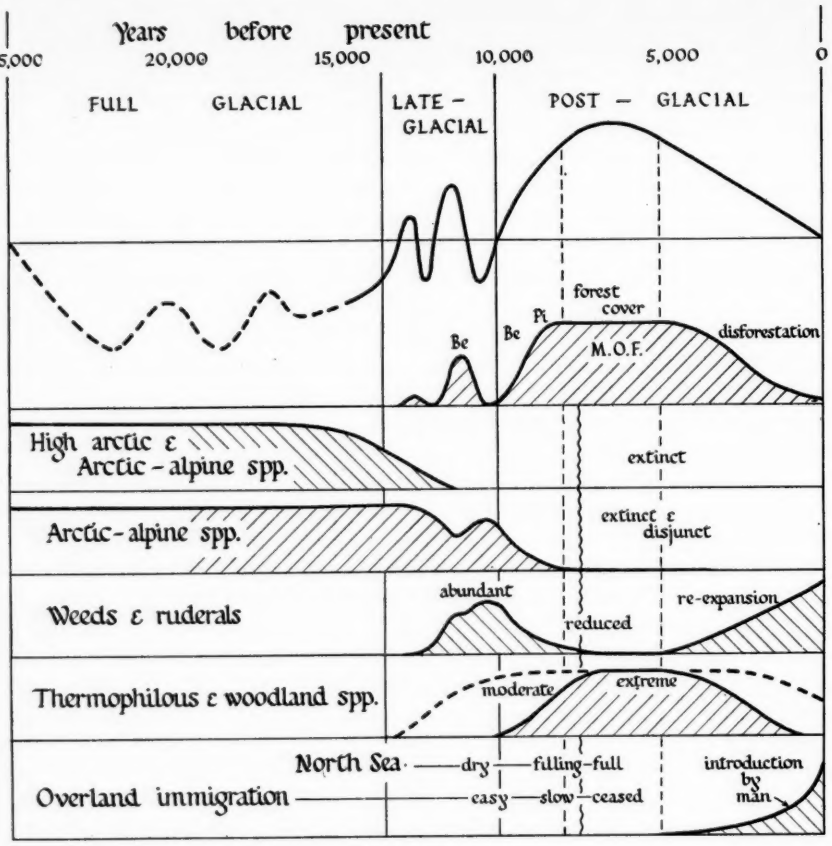

Figure 2

Let it then be acknowledged that we have this substantial knowledge of the historical factors involved in the present ranges of many categories of our flora; it nevertheless remains to answer the question of whence they came. If the British Isles in the Full-glacial period had too harsh a climate for tree-growth and its vegetation was open grass-sedge communities with arctic birch and willows, it is unlikely that any but a small proportion of the total flora was present here, and we are forced to ask where it came from, and at what time.

Such questions raise at once the decisive issue of the part played by the North Sea in isolating us from invasion from the European mainland.

During the maximum of the glaciation so much water was locked up in the ice-sheets of the world that the ocean level everywhere fell by some hundreds of feet. This eustatic fall in level laid bare the bed of the North Sea, and evidence for its freedom from the sea is shown by the freshwater peat (moorlog) recovered from the Dogger Bank and other parts of its floor.

It will be noted that when climate improved at the close of the last glaciation three processes were initiated:

(1) melting of the ice-sheets, their retreat and the exposure of fresh country to colonisation;
(2) restoration of the ocean-level by return of water from these ice-sheets;
(3) the northwards migration of vegetational belts, of flora and fauna driven south by the oncoming cold.

It is clearly of the greatest importance to the biogeographer to know the progress of these three processes in relation to one another (and if possible their absolute

timing). In particular we need to know for the British Isles the date at which the North Sea was last available as a dry-land migration route to this country. This is fortunately a matter which can be resolved by pollen-analyses of submerged peat-beds from the deep floor of the North Sea and from less deeply submerged coastal peat-beds. Without entering into the evidence it may be said at once that a very rapid rise of sea-level was in progress in our pollen-zone VI, and that during this time, which can be placed perhaps at 5000 B.C., the isolation of the British Isles was finally and rather quickly accomplished. If the major restoration in ocean level was in fact caused by melting of the ice-sheets, it seems at first sight unreasonable that this restoration should not have taken place until 3,000 years after the last British Ice had vanished. This in fact need not seriously disturb the argument, for at the time of disappearance of the British Ice, we know with certainty that the great North American ice-sheets extended south to the Great Lakes, and larger ice-sheets of the world may equally well have persisted long after the small British ice-fields.

Now by reference to our diagrammatic scheme of forest development it will be apparent that by the middle of Zone VI the climate had become mild enough to have ushered in all the components of the mixed-oak-forest, and we may hazard the conjecture that it is not necessary, even for our most thermophilous species, to suppose that they had to enter the country across sea-barriers.

None the less, it is reasonable to enquire what *is* the positive evidence for the progress of immigration of the British flora. If we make a careful enumeration of the number of species of our present flora recognised in each part of the Full-glacial, Late-glacial and Post-glacial periods, it is possible to construct therefrom an increment graph showing the totals known at any given time to have been already recorded in the country. Our first approximation shows that such dated records exist for over 500 species of flowering plants out of the total number of perhaps 1,500 species likely to be recognisable as fossils. If this third of our flora is a guide to the behaviour of the total flora, as it must surely be, then we can state certain quite remarkable conclusions. In the first place it then appears that a very high proportion of our flora was already present in Late-glacial time, at least 50 per cent. In the second place, by the end of Zone VI, when the North Sea had just filled up, at least 65 or 70 per cent. of our flora was already here. If it is agreed that man may have been responsible for introducing three hundred or four hundred species subsequently, we are left with a very significant conclusion. That is that there is little or no need to invoke any overseas migration phenomena to explain the presence of our native flora in these Islands. Nor, as we have seen, need we suppose per-glacial survival, although this may well have applied to a hardy component. It is now evident that the bulk of our flora could have immigrated by dry-land routes from the Continent in the period between the glacial retreat and the final restoration of our insularity.

The very great advantage of realising this situation is that it provides for the effective action of sea-barriers to plant dispersal over a substantial time period: this conforms to general ecological and phytogeographical experience, and equally with the sharp break in numbers and kind between the West European flora and our own. It indicates that there was a dry-land stage when conditions favoured extremely rapid immigration and establishment, and a later one of insulation and isolation. The significance of such a conclusion for problems like those of the Hiberno-American and Hiberno-Lusitanian floras is evident, but space forbids that I should now attempt to show how the available fossil evidence is not at variance with an explanation in these terms.

This very brief survey completed, I would say in conclusion that investigations of this kind persuade me to advance the following principles for the study of

biogeography in the British Isles: the word 'biogeography' is used advisedly, for I see no reason why they should not apply equally well to animals as to plants.

(1) The facts of geographical distribution of plants and animals can only be interpreted in the light of knowledge of former geological and climatic conditions.
(2) To this end, it is particularly necessary in this country to learn as much as possible of the conditions of the Last Glaciation, of the transitional Late-glacial period, and of the final and still-continuing Post-glacial period.
(3) To be sure of the meaning of present distribution patterns it is not enough to speculate from the forms of the patterns: we must have direct fossil evidence of the antecedent distribution of the species.
(4) Such evidence from sub-fossil material in dateable deposits is abundant and will quickly allow rejection of some hypotheses and substantiation of others.
(5) The impress of the Last Glaciation and subsequent events is so strong that it is unlikely that existing biogeographic patterns in this country disclose any evidence of having been determined by earlier glaciations or interglacial periods.

From studies of the kind I have mentioned a sufficient picture has already emerged of the true history of our flora for us to say that in place of slender conjecture we have substituted a factual basis for phytogeography.

THE IMPACT OF BRITISH EDUCATION ON THE INDIGENOUS PEOPLES OF OVERSEA TERRITORIES[1]

ADDRESS BY SIR CHRISTOPHER COX, K.C.M.G.

PRESIDENT OF SECTION L

THE honour paid to me by the invitation to deliver your 1956 Presidential Address springs, I feel sure, from the increasing recognition in late years of the British contribution to education in what are still called the colonial territories. Relatively few of us even now are directly concerned; but there is a feeling that this work has been important, distinctive and not wholly discreditable, that it will not go on for ever, and that it is now at a stage when it should be more widely known.

The situation, so familiar to us, in which one people finds itself ultimately responsible for the education of other peoples of very different stock in other parts of the globe is, when you pause to consider it, peculiar. The responsibility, while it lasts, is formidable: it has fallen to few peoples, and to none I think on quite so large a scale as to our own. The nature of this educational responsibility, and the way in which we have been discharging it, should therefore be of the deepest interest to all at work on education in this country. And this is more rather than less true at the present time, when the colonial epoch is moving towards its self-fulfilling close. The colonial relationship itself will soon be a curiosity of history. For a long time now our colonial policy, however mixed its origins, has had self-government as its objective, and fundamental in that policy as a nation-building agency has been education. The tempo of progress has been transformed in the post-war world: the orderly trot has broken into a breathless gallop: peoples whom we have long guided are one by one obtaining independence or passing through successive phases of self-rule.

But by a happy and far from accidental paradox, as the time when these young countries will be running their own affairs gets closer and our direct colonial responsibilities in education contract, the calls upon this country's educational resources visibly multiply: the number of oversea students over here, of educational visitors, of requests for educational workers and counsellors has mounted by leaps and bounds. For the colonial relationship, as it fades out, is, if all has gone well, replaced by that of a continuing association of special intimacy, and more of us than ever before may find ourselves called upon to take some part. All the more important is it therefore at such a time to spread and deepen understanding in this country.

In this short address I cannot range over the whole colonial field in its vastness and variety. I shall be speaking primarily, therefore, of our educational relationship with peoples of other cultures already resident in colonial territories when our responsibilities began. I shall not be discussing the education of Europeans in the plural societies of East or Central Africa, for their culture is our own. What I have to say, too, will be only partially applicable to the West Indies, so many of whose people came there with us and have, so to speak, grown up with us, or to those plural societies, like Malaya, where other non-European peoples have brought their cultures with them mainly or entirely during the period of colonial rule.

I shall first glance at some of the changes in our own attitude towards education in this field, and then consider more fully the fundamental factor in this sort of

[1] Address delivered at the Sheffield Meeting of the British Association on August 30, 1956.

colonial educational relationship, namely, the impact of western schooling upon traditional societies, and some of its consequences. Against that determining background I shall allude, briefly, to the resulting educational situation as it is today—of which we shall hear more this week—with special reference to the kind of help we can most effectively give in this period of transition to the post-colonial epoch.

In emphasising the transition from the epoch of this country's responsibility for educational policy to that of self-government and, one hopes, of a continuing professional intimacy, I must not suggest that a coherent educational policy was formulated in very early days by British officials overseas, let alone by the Colonial Office in Whitehall. Far from it. The Governments of the many scattered, diverse and as a rule wretchedly poor colonial territories had neither revenue nor personnel to initiate an active educational policy; instead, as happened in this country, the initiative was in general left to the churches, whose schools for a long period in most territories, particularly in the Caribbean and in Africa, met the modest needs of Government and commerce for literate employees. In the early days it was the missionaries and not Colonial Governments who in practice determined the way in which western schooling should be introduced to the colonial peoples; and the pattern and methods which they followed reproduced the familiar models they had known in Britain—though always, of course, scaled down to the pitifully little which was all they could afford.

Here and there, exceptionally, early planning by a Colonial Government itself was brought about by such special factors as the presence of a powerful non-Christian world religion, severely limiting missionary activity. The dominance of Islam determined Lord Lugard's educational policy among the Northern Nigerian Emirates; so, too, in the Sudan which, like India on its infinitely greater scale, was basically in 'colonial' relationship with this country. In India itself the strength of Hinduism and Islam was hardly compatible with laissez-faire in education, and Government deliberately formulated the official educational policy, always associated with Macaulay; thenceforward a powerful centralised bureaucracy was working to a definite policy, with far-reaching though not always intended consequences.

But it was only after the first world war that a more general 'colonial educational policy' took shape. The concept of a Commonwealth of Nations, comprising the Dominions, had been proclaimed; in India a nation-building policy, made more explicit, had come nearer fulfilment; the mandatory principle was enshrined in the post-war settlement. For the first time the scattered heterogeneous miscellany of colonial territories, great and small, received attention and continuous study from a single standpoint. It was in this changed climate that the new interest in colonial education came to birth. The focus was first on Africa. Churches, Government, American philanthropy, eagerly played their part. The Phelps-Stokes Missions visited much of Africa between 1922 and 1924 and reported, vividly and fully. Action was prompt. The Advisory Committee on Native Education in Tropical Africa, soon widened to become the Advisory Committee on Education in the Colonies, was set up in London, and presented the Secretary of State with a basic memorandum on Educational Policy in Tropical Africa (the famous White Paper of 1925), followed by a succession of reports working out different aspects in more detail. The Colonial Educational Service was established; Dr. Percy Nunn and others devised special training for its recruits. Before long the Advisory Committee was discussing policy, at its monthly sessions, not only with Governors from Africa but with the heads of the newly constituted Education Departments, and was beginning to submit comments upon their Annual Reports. From then onwards the Secretary of State for the Colonies has drawn freely upon the sustained interest and

counsel of this distinguished consultative body. The Committee has included many Past Presidents of this Section, and has had at its command experience from the Universities and their Institutes of Education, from H.M. Inspectorate, the Local Education Authorities, the Church organisations, the National Union of Teachers, from public schools and grammar schools, technical colleges, training colleges, the W.E.A., from education in Scotland and in Wales. For thirty crucial years the voluntary work of some of the best professional minds in United Kingdom education has been dedicated to building up education in the colonial territories, increasingly with the goal in mind of ultimate self-government.

During these years, and especially since the war, the growth of the colonial educational systems and the maturing of their professional standards, together with the emergence of strong public opinion and the movement towards self-government, have progressively reduced the need for formal advice from the Secretary of State. On the other hand the same factors of educational growth and national emergence have multiplied occasions for contact, for consultation, and for practical help, when the guidance that United Kingdom experience can provide is felt to be really needed.

Most conspicuously has this been so in the field of university education. The Asquith Commission's report in 1945, gave the necessary guidance and has been the inspiration of the most sustained and dramatic contribution which this country has made to colonial education since the war; one university and six university colleges have been born in these years. The Colonial Development and Welfare Acts made these, like much else, possible; but advice on policy as well as practical assistance derives from two academic bodies, the University of London on the one hand and the Inter-University Council for Higher Education Overseas representing all the United Kingdom universities on the other, and this has made all the difference to its acceptability. The traditional independence of British universities eliminates any suspicion that their help might mask continued dominance by Whitehall.

But help in university development is only the most striking single example of the way in which this country's educational experience is still being drawn upon. An ebullient and increasingly independent colonial world is leaving Whitehall control far behind it, but its educational systems, derived almost exclusively from this country, are being put to every kind of test and strain. The day of the despatch, directed to where we ourselves may have judged the need to lie, is over; but the visiting mission or specialist, invited to give professional advice or help, has never been more in demand. To some extent the Secretary of State's own team of Educational Advisers, who try to keep abreast of developments here, can give such help on their constant tours. But the visitors who can nowadays help colonial territories most will not have the taint of colonial officialdom; they should combine specialist experience in some one field with an understanding of the United Kingdom educational scene as a whole; and they should be sensitive to the different conditions, presuppositions and aspirations of countries which, as they grow up, are trying to shape to their own needs an educational heritage that has been brought to them from far away.

It was in that spirit that in 1951, a quarter of a century after the Phelps-Stokes survey, the two Study Groups, led by Dr. G. B. Jeffery and Sir Arthur Binns, and sponsored by the Nuffield Foundation in partnership with the Colonial Office, spent many months in West, East and Central Africa before producing the reports which were discussed at the Cambridge Conference on African Education under Sir Philip Morris's chairmanship and are incorporated in the volume *African Education*. That conference was attended by West African Ministers and their European Directors of Education as well as by European and African educational administrators and teachers, whether government or missionary, from all over

British Africa. Men and women carrying heavy political, administrative or professional responsibility discussed with each other and with discriminating professional observers from the United Kingdom problems that had in common the basic necessity for adapting an educational system derived primarily from Britain to meet the distinctive needs of emerging countries in Africa. And there is a story of similar, though much less elaborate, contacts elsewhere.

So far I have been indicating, very broadly, the main stages in our changing approach to these educational responsibilities. I now want to try to bring out certain peculiar factors in the situation, derived from the colonial relationship itself, which are important both for understanding the background of current trends and for making our contribution most effectively in these later stages of the colonial enterprise and in its sequel.

It is of the essence of the colonial relationship that two different cultures, or ways of life, are brought into close and continuing contact and that change in the indigenous people's way of life will result. Such changes are brought about by contact with the ruling people in all kinds of ways, but none of these is more potent than the introduction of western schooling. We cannot begin to understand the educational situation in these countries, unless we consider briefly the effects of the importation of western schooling as an instrument of violent social change.

For the colonial context there is a startling relevance in the familiar antithesis between the school as an instrument for transmitting and conserving a society's or a social group's way of life and the school as an instrument of change. It may be that the conscious planning of any educational system with either of these aims in view is, historically, sufficiently rare to be always interesting; but each is exemplified in extreme form in colonial education.

In the deliberately conservative approach such paramount importance is attached to preserving intact a particular way of life that the whole upbringing, formal or informal, of successive generations of youth is designed to that end, characters and minds being so fashioned and set that the group's distinctive life will be perpetuated. We think at once of, say, ancient Sparta, with its rigorous and minutely regulated system that fascinated the rest of the Greek world; or of the knightly class within the feudal society; or of the rigorous educational codes by which the rulers of totalitarian states seek to perpetuate the new societies they have created. But no more striking examples of a conservative approach are to be found than in many of the preliterate types of traditional society for which the modern colonial power in the later nineteenth century was constantly finding it had become responsible. In a paper read to this Section at Edinburgh in 1951, Professor Margaret Read referred to some of the studies of the elaborately conservative training given to children by preliterate societies among the North American Indians, in the Pacific or in Africa. Enough is known to warrant the axiom that not only will any identifiable tribe in its traditional state have such a conservative system but that without it the tribe's identity would begin to disintegrate.

At the other extreme it can be a deliberate policy to use the school system as a principal instrument for changing the whole outlook and habits of a country or of a class. We may think first of Prussia after Jena, of the U.S.A.'s policy for the assimilation of immigrant stocks, of the rulers of totalitarian states when they first seize power; but most relevant from our standpoint will be when the leaders of a people that has remained isolated decide to re-educate their people so as to bring them into the stream of western civilisation, like Japan in the late nineteenth century or Turkey after the first world war.

In the colonial situation the formidable power of western schooling as an agency of change has from the outset been a fact of the first importance. The missionary

societies who introduced western schooling into so much of the present Colonial Empire did so as part of their single-minded effort to transform the traditional way of life, while in its own very different way the Government of India had set out with a not less drastic though wider aim. 'The languages of Western Europe', wrote Macaulay, in which 'the greatest mass of information' had been laid up, 'civilised Russia. I cannot doubt that they will do for the Hindoo what they have done for the Tartar.'

The use of education as an instrument of change in this way is one thing when sponsored by the people's own spokesmen. It is quite another when sponsored by a distant ruling power or by the emissaries of churches centred far away. The efficacy of any system of education depends, as Mayhew has emphasised in his study of education in India, on whether the community concerned is inspired, however vaguely and subconsciously, by the same aims and ideas as those who control the system. Any drastic educational policy, even in the hands of the community's own leaders, will not have the results it intends unless it in large measure reflects the aims and aspirations of the community concerned. It is important that anyone studying the colonial educational scene today should bear in mind that what has come about results not only from the introduction of western schooling to peoples with a different way of life, but from the fact that its introduction had to be planned from above by members of a ruling race from oversea. We are a long way in spirit from John Lawrence's proud boast: 'We are here by our own moral superiority, by the force of circumstance, and by the Will of Providence. These alone constitute our charter of government, and in doing the best we can for the people we are bound by our conscience and not by theirs.' But until very lately both our motives and our machinery have had to be those of benevolent paternalism, with little access to the people's minds, or from theirs to ours. In many territories, until very recently indeed, John Lawrence's conscience has still been marching on.

The effect of introducing, from outside and from above, western schooling into strongly traditional societies has been to bring about a sharp divergence between two forms of education, foreign contrasted with traditional, formal with informal, that of the school with that of the home and of the community, education for livelihood in contrast with that for living well. Mayhew and Furnivall have analysed the failure of the British educational systems in India, Burma and elsewhere in the east to penetrate patterns of living rooted in religious systems 'that pervaded every detail of social and domestic life'. British rule had brought to a widening number new avenues for material advancement to which the entry was the western schools. Thus the latter, though by western standards the curriculum was liberal, became for parents and children primarily vocational schools; 'their spiritual home was elsewhere, in their family circle and among a community untouched by western ideas'. For this school-going minority, therefore, life fell into two compartments, co-existing but not fused; while, sealed off from the English-medium schools and colleges of the towns and the outlets to which these led, such elementary vernacular schools as could be afforded for the countryside were ineffective and unpopular, leaving virtually untouched the main stream of its multitudinous life. To the great benefit of their country many Indians took the fullest advantage of the English-medium schools and colleges as avenues for economic and political advancement. The impact of western culture stimulated and enriched a number of their finest minds. Yet, as late as the 1920s, Mayhew could sum up that India 'looks to our schools and colleges for equipment in the struggle for existence; for the secret of happy living, she looks elsewhere'. Moreover Lord Curzon's efforts to relate rural schools to their environment and to use them to improve it, by emphasising hygiene, civics and rural science, had made no impact. The people who were rooted in that environment knew by then what they wanted from schools, and it was not this.

With that history of British education in the east to study, and with far greater anthropological knowledge, those who drew up the 1925 White Paper for Africa did their best to spare the African peoples, in the course of their gradual transformation under British rule, those deep-seated but wholly unintended distortions and injuries to which our lack of experience had contributed elsewhere. They sought to avoid two major dangers that might result from the extraneous plastering on native society of western schooling in the form it had then assumed in Britain: on the one hand, cleavage within the personalities of the pupils if school became the highway to a livelihood while the pattern of how to live and springs of character were still derived from home and tribe; on the other, cleavage between the class who had received western schooling and the rest of the community. Keynotes of the White Paper were adaptation and the grafting of western schooling on to indigenous stock. The greater efficiency of the individual is linked with the need to utilise him in promoting the advancement of the community through all-round community betterment, by way of agriculture, health, native industries and local government. Vernacular languages are to have their proper place in schooling. Character is throughout seen as, with efficiency, the principal field and fruit of education. There is therefore preoccupation with the importance of religious teaching, and a great effort is made to preserve even after the revolution of Christian conversion all that has not been harmful in the social system. With this in mind the White Paper welcomed, subject to the general direction of Government, the continuance of the heritage from the days of laissez-faire by which the overwhelming preponderance of western schooling in all but Moslem areas had long been provided by Christian missionary bodies. The Committee was keenly alive to the danger, in such a setting, that the spiritual sanctions dependent upon local cults might collapse without replacement, under the impact of western schooling.

The White Paper was adopted and proclaimed. The Phelps-Stokes Reports, published at about the same time, spread widely a similar philosophy. African schooling was to be rooted in the progressive control of the environment; more advanced schooling and training were to produce the educated agents for this work.

Such were our intentions. To what extent were they fulfilled? Intentions and fulfilment are very different things, particularly when those responsible for policy and those for whose benefit it is designed have very different aims and values. A great deal that has turned out unexpectedly in African education flows from the conviction of capable, keen and self-confident professional men and women from this country that this or that part of their policy was the best for the people of the country they were serving, and from the fact that the people themselves did not share that view. The African attitude in those early days, with little outlet for expression, was often undervalued, sometimes overlooked; yet, like that of colonial peoples elsewhere, it was always important, and was to become decisive. Our effort to apply to African educational policy lessons learnt from experience in India and the East probably came twenty or thirty years too late. For western schooling, however thinly spread and rudimentary, had already taken root. African reactions to this new and arresting phenomenon had begun to take shape long before the wisdom of the Advisory Committee or Phelps-Stokes. That having once happened, the fundamental, though often underrated, factor in African education, as in colonial education generally, is less the policy of government than the attitude of the governed. 'In the long run Africa will take what it wants, digest it slowly, and assimilate what it does not spit out again.'[1]

Malinowski first brought out the importance of the reactions of traditional societies to the changes offered them by the dominant culture of a ruling race. Professor Margaret Read drew this Section's attention in 1951 to certain stages of development in the reactions of such societies in Africa to western schooling.

[1] *The Round Table*, No. 157, December 1949, p. 53.

There is an initial stage of conservative resistance, when schooling like other things western is viewed with suspicion or hostility as a threat to the familiar traditional way of life and when parents have to be ordered or bribed to send their children to school at all. That is followed by a stage of gradual and selective acceptance of some of the new elements learnt through the schools, with a corresponding disintegration of some of the traditional elements with which these seem incompatible —traditional songs, dances and folklore, for example, giving way to hymns, drill and 'readers'. Then comes the third, and dramatic, stage of total acceptance of what is believed to be a complete western schooling, identical with that of the British rulers and guaranteed by common examinations; indeed there is now, so far as possible, insistence upon this in its totality. Only much later is a further stage reached, after there has been full conviction that there are no barriers on the part of the ruling power to total adoption of all that is wanted from the western system and this has been itself built into the social fabric. Thereafter those who have most fully assimilated western culture may be ready to give the lead in partially reinstating certain elements of the traditional culture in the educational system: and this stage is a natural prelude to the achievement of that distinctive national identity, cultural as well as political, which has for so long been the implicit objective of our educational policy.

This background helps to explain why the Colonial Office educational policy of the twenties, inspired by such admirably conceived principles and promoted so earnestly from London, yet fell short of comprehensive fulfilment. Many factors were involved, which there is no time to analyse today: the lack of resources, for example, for establishing more key institutions in which the practical application of the principles could be worked out; the inadequate equipment of some of the missionaries of many nationalities and denominations; the lack of educators with anthropological training. But the underlying factor is that of African response. The reactions of Africans to the importation of western schooling had in some areas already reached, and were in others on the way towards, the third of the stages I have outlined: that of total acceptance of, and insistence upon, all that they wanted from such schooling. No policy from Britain could put that process into reverse. The demand, unformulated perhaps but deep-rooted, was for access to all that Britain had to offer. Selection there might have to be: but selection by Africans, in Africa; not for Africans, in Britain. A strongly selective process was in fact at work on the African side, determining which features of western schooling were accepted by them as authentic. When schooling is the route to salaried employment, and Governments recognise the paper qualifications of examination results as the test of schooling, is it surprising that the learning of examinable information becomes all-important? that curricular activities not easily examinable should seem irrelevant? that the whole process of character-training appears to lack any necessary connection with western schooling? or that the principle of adaptation to environment should be suspect from the start? All such principles can so easily appear as devices designed by the ruling power to keep the African back, at the very time when he is beginning to surge forward. Again, the educational effort must in any case be contracted by lack of resources: why therefore fritter these away in the pursuit of unsolicited and barely perceptible improvements to village life, instead of giving priority at all costs to the building up of the western school system? What set fire to the imagination of the Gold Coast and evoked an ardour that has never abated was the founding of Achimota at just about that time; not, I feel sure, because Achimota broke new ground in secondary education by seeking to build on indigenous tradition but because it led on to the highest qualifications and became the symbol of advance.

All this relates to Africa, and the fact that nearly four-fifths of the indigenous population of our oversea territories are found in that continent means that their

problems are bound to occupy us chiefly this morning, but what is said of them now is broadly true, I think, of territories with strong traditional culture elsewhere. The general picture all over British Africa, despite its many remaining backward areas, is today increasingly one in which the African people have set their hearts, with an impatient fervour that is hard for us to realise, upon a complete western education as they believe it to be. In West Africa national feeling has reinforced the urge for individual material or cultural advance; educational progress, both outward to the masses and more particularly upwards for the nurture of leaders and professional men and technicians, has been identified with the post-war march towards self-government. It is that background that determines the acceptability of what we have to offer today. For the most part this is of course eagerly welcomed. But the purest educational advice from this country may still go astray. The doubts that we, once so sublimely confident, feel as to the effortless superiority of our own culture in this respect or that, the self-questionings that have prompted improvements in our own educational approach, come at a time when the African confidence in its superiority and in his own capacity to master it, if he can only get the chance, is supreme. Remembering our eloquent insistence upon adaptation, he may regard with suspicion the arrival of new ideas as specially fashioned for the African export market, if these deviate from the familiar pattern which we exported to him earlier and under which he knows that we ourselves grew up. He may associate our educational idealism with political conservatism, and indeed each has at times found the other an ally, for to both the best developments need time and cannot be rushed. Our post-war higher educational policy has of course been open to no such doubts. Towards technical education the attitude is transformed when it is seen as a doorway into the modern world. Projects for developing trade schools and training artisans have not won general support if they are suspected of being the policy of a ruling power more concerned with the supply of hewers of wood and drawers of water than with programmes of advancement. With economic development or the approach of self-government, the whole outlook can change very fast. West African Ministers are impatient to set on foot the kind of comprehensive technical education programmes that will enable their countries to become, as one of them put it, centres of their own expanding economy instead of a market and source of supply for raw materials on the periphery of the economy of the ruling power.

The British contribution depends, therefore, for its effectiveness today largely on the extent to which we can understand what the peoples themselves want and can come to terms with this. That is obvious enough where, as in West Africa, the achievement of self-government means that local views and wishes on education are recognised as a highly important political factor by Ministers responsible to an electorate with very strong feelings on this subject indeed. A crucial test of our approach to the colonial relationship may then occur. For in such countries there is likely to be a transitional stage, after colonial official rule is over but while Ministers must still rely upon British officials. The latter's help is needed as never before, and their influence will be in proportion to their insight and goodwill. But also, in the kind of setting where colonial conditions persist, and we still have responsibility for policy and for professional leadership, what matters most is whether in discharging this we can harness local sentiment and carry with us those who teach in and use the schools. It is not that we should refrain from giving that which justifies our presence: it is that we should study the conditions in which what we have to give will bear most fruit.

Against this background certain features of the colonial educational scene today stand out.

In most territories the demand for schooling has now reached a stage of intensity,

at times almost obsessional in its strength, to which there has been, I think, no counterpart in the history of the United Kingdom, at any rate east of the Severn and south of the Tweed. As long as demand more or less kept pace with resources, the problem of bringing western schooling within reach of the maximum number, while securing the qualitative standards proper to an emergent country, did not become acute. But today the demand altogether outstrips the capacity of the territories to meet it. The fact that their resources bear no comparison whatever with those of an industrial country inexorably governs the whole approach to educational development and limits this to a fraction of the comprehensive system reached by a modern state after centuries of growth. Yet many of them are moving rapidly towards self-government.

The symposium tomorrow afternoon will discuss, from various angles, the question whether the all-important economic development, from which alone this quite disproportionate educational demand can be met, is accelerated or retarded by giving priority to educational investment. A further symposium at our penultimate session will review the central educational dilemma in which these territories find themselves in the meantime, with the rival claims of quality and quantity, in a setting of exiguous resources and insatiable demand. Most of what I have still time to say this morning is by way of preliminary to those discussions.

With popular self-government we naturally find that the satisfaction of this peremptory demand is given top priority in programmes for universal free primary education. Of such programmes Gold Coast and Western Nigeria provide the two most striking examples. Both are already sufficiently remarkable for each to have its place, whatever the ultimate issue, in any history of educational expansion. Of such programmes one asks two questions: Can it be done, financially and administratively? If it can, at what price in loss of standards?

The short answer to the first question is that it has been done so far. Administratively it has been the supreme test of the British officials carried over into the ministerial regime, and they have responded magnificently to an opportunity for which only an African Ministerial Government could have given financial sanction; under the Colonial official regime the principle of giving priority to revenue-producing services would have meant the postponement of universal primary education until economic growth had gone much further. But the recurrent expenditure entailed is cumulative; the policy of turning local enthusiasm to full financial account by building up local education authorities is still in the experimental stage; the programmes are still unfolding, with consequences in the subsequent demand for secondary school expansion that it is impossible to predict at this stage. Some drop in primary standards there has inevitably been. So far it has not been catastrophic. Many steps have been taken to mitigate the damage done and to restore the position, by a big expansion of teacher-training and in several other ways.

Dr. Jeffery's Study Group considered the predicament of educationists charged with helping these young countries to move from illiteracy to universal schooling. Purely professionally, there is everything to be said for steady progress at a pace governed by a country's capacity to pay for the supply of trained teachers and of good buildings and equipment. In practice the educationist is either under the economic pressure to go too slowly or under the political pressure to go, from the standpoint of quality, too fast. The Groups' view was that it is important to establish standards before the stage of mass demand is reached; once this comes, it is better passed through as rapidly as possible; consolidation and the re-assertion of standards follow. Economic factors will determine the time this requires.

Once this mass expansion occurs, the problem becomes closer to that in the West Indies. There, in spite of acute and chronic poverty and the handicap of a population increasing much more rapidly than revenue, different historical factors have

meant that all who wished to go to primary schools have for long been provided for somehow or other in the public system, inevitably in conditions which we should regard as for the most part shockingly bad. In spite of all that there is a most notable West Indian cultural achievement. In much of Africa, however, a strong imported Colonial Educational Service, with backing from London, long felt bound to stand out for minimum standards as regards ratio of trained staff, buildings and so forth, thereby limiting the facilities provided in the public aided system and leaving those left outside to shift for themselves, probably initially through bush schools and later through private schools of various kinds. When the stage of mass demand is reached an acute problem arises, and under a popular government that can only be solved, whatever the consequences, by stretching the public system to provide universal free primary schooling for all of the right age who want it. As long as the revenue per head is so much lower in most African territories than in this country, the resulting educational provision is bound to be very different from our own. Our duty is to have done our utmost to enable the colonial peoples to embody in some part of their systems our own standards, to introduce them to the general pattern of the system that we know and believe in, to suggest ways in which we ourselves think that system can be adapted to local needs, and then to give the fullest weight to their wishes.

Let me emphasise certain fields in which what we have to offer must be first-class, without qualification, if the health of these young educational systems is to be assured. If these standards are later judged to be too exacting, it is much easier for their leaders to relax them than to follow the reverse process. It would be unforgivable in a colonial power to withhold the opportunity of achieving first-class standards. There can be no doubt that on this our policy has been regarded as right and widely welcomed by those whom it affects.

At the apex stand the universities. Here, since the war-time Commissions, policy has been unambiguous. The emphasis has throughout been upon high standards and the Inter-University Council's unswerving principle has been that 'no greater disservice could be rendered . . . than to set up an inferior institution and call it a university.' Special relationship with the University of London not only safeguards standards: by arrangements for adapting courses to include local material, and in other ways, it is designed to prevent the requirements of an academic society thousands of miles away from causing educational distortion.

Secondly, secondary grammar schools. Here what matters is that some really good schools should be built up during the relatively leisured period before the demand for mass expansion, and that when that time comes these should be strengthened rather than weakened. The readiness of the Cambridge Examinations Syndicate to foster the principle of adaptation within the guarantee of protected standards reconciles external examining with educational principle in ways acceptable to local opinion.

A third field in which our contribution should be of decisive importance is that of teacher-training. Primary education has, as we have seen, its own level to find and its own adjustment to local requirements to work out. To all this the training colleges should be in colonial conditions quite peculiarly the key. With us much of the initiative for improvement has traditionally sprung from the individual working teacher. So often in colonial conditions the great majority of primary school teachers have not had a secondary education and are teaching at the very limits of their knowledge. Visiting teachers may help, but hardly anywhere as yet can the influence of a purely professional inspectorate, freed from administrative duties, be brought to bear at the primary level. The training college therefore finds itself with a responsibility which it cannot easily share. Moreover western schooling has been so recently imported that it lies on those who know it from the inside themselves to take the initiative in interpreting it and in adapting it to the needs and prejudices

of the people. This means that it is primarily at the better training colleges that the really fundamental work of introducing new ideas and methods must be undertaken, and not only the teacher's understanding elicited but his conviction and zest; for without these no real change in the quality of primary education will be effected. This kind of training college is therefore always breaking new ground, and should be the source of inspiration for primary education throughout the region. It comes about only when some of the best professional minds from both countries are associated in the closest of working partnerships.

I should like to have had time to bring out how much depends on this country's response to certain other requests for help that are likely to increase in volume and urgency. I am thinking of technical education, to whose crucial importance in equipping these young countries to take their place in the modern world there will be reference in tomorrow's symposium; of the education of girls, so prominent and indeed elaborate in the training of many traditional societies, so much retarded when western schooling first comes into the picture (and for that very reason so dependent on the continued help of women from this country), and yet so fundamental in the achievement of a unified educational system with the influences of home and school in mutual support; and of adult education of various kinds, so pressing when the school system is still so pitifully defective and when in a time of such rapid transformation and such slender economic resources it is essential to do something to bridge the gap between the generations.

So I return to my starting point. The nation-building relevance of our educational contribution stands out most clearly in the closing stages of the colonial relationship, and it is also in these closing stages that in some ways the demands upon this country's help are most exacting. There is no longer the responsibility for planning policy from London; nor is there now so often recruitment for the fascinating long-term job of pioneering work without the pressures of rapid change. The demands now are of a different kind. Paradoxically, as self-government draws nearer, the great growth of the educational systems has led to a quite startling increase in the call for staff, many of them short-term, from this country. Equally startling has been the increased flow of oversea students to this country (over 11,000 are here this year), and at the same time there is now a stream of mature professional visitors wishing to see for themselves this or that feature of our educational system, schools or perhaps the working of L.E.A.s, the N.U.T., the Inspectorate, the Ministry. The two-way movement is completed by the increased number of calls for *ad hoc* missions from this country, such as those of the team of H.M.I.s who have been helping Malaya to form an Inspectorate or the small apostolic group that went last year from the secondary modern schools of Hertfordshire to run a special course in Western Nigeria.

Our colonial educational heritage has therefore never been more with us or been a matter of personal concern to more of us in this country than it is today as our responsibilities draw to a close. They want us to go out and help them, and they come over to us partly because they are aware that the questions needing answers, arising in the form in which they do arise, derive from the system that we have carried to them and that they have welcomed, but partly also, I feel sure, because they do still think, however far the process of self-government has gone, that we have many of the answers. When they visit this country we can help them most, I think, if we have some awareness of the way in which we have been mixed up together. Let us bear in mind that we have been the instruments of history in setting in motion a transformation in their countries, both disruptive and constructive, more drastic and more rapid than any we ourselves have known. Let us recall that, in spite of links between us and of the fact that they have insistently

built into their systems so much that comes from Great Britain, some of them are passing beyond the stage when they were determined to acquire uncritically all that we could offer, and are now making comparisons and prepared, rightly, to look elsewhere as well. Let us not forget that their revenues are still fragmentary compared with our own, and that their own educational systems reflect this, so that we do not concentrate on displaying our most costly new special schools or gadgets instead of showing them what they probably most want to see, the essentials of our system and how it works. Let us give a balanced picture of what we are doing and make it clear, though without mock modesty, that we are learning from experience all the time.

That, too, I think, is the kind of attitude that achieves most when the movement is the other way and we ourselves go overseas. But then there are infinitely greater opportunities for us to study and learn the way of life, the pattern of thought and of feeling, of those whom we are trying to help. Once more our help will be greatest if we recall that our problems and theirs, despite many similarities, are not identical; that our assumptions at any given time, about which we can be so very confident, may not have a universal validity; and above all, perhaps, that, although our experience, knowledge and insight into educational principle should mean that we have a great deal to offer, no real educational advance will occur unless there is not merely compliance or courtesy on the part of those who are working with us, but comprehension, conviction and enthusiasm, and that this can be obtained, if we are working with and for a people not our own, only through intimate understanding and partnership.

The verdict of history on our colonial educational work cannot yet be given. We may perhaps feel that our work as colonial educators is finished when we have offered to the colonial peoples the best that we know and helped them to assimilate or to adapt such of it as they wish to have. But the way in which we do this, particularly in these final stages, will determine whether the educational links that will continue between many young countries and our own will only be those of history, or whether there will emerge a new and active relationship of mutual benefit among equal partners, let us hope within the Commonwealth.

THE INFLUENCE OF MAN ON SOIL FERTILITY[1]

ADDRESS BY G. V. JACKS

PRESIDENT OF SECTION M

By Man, spelt with a capital M, I mean human societies, including men and women, cows, crops, microbes, cars, steelworks and stock exchanges. By soil fertility I mean the capacity of a soil to produce living material, regardless of how the soil acquired that capacity. If one man treats a spoil heap with fertilizers he may produce a fertile soil within a year, but the fertility will be evanescent, and he will not find it worth his while to maintain it. But Man—human society—may judge it worth while to reclaim the spoil heap for permanent agriculture, in which case he sets in motion some long-term processes largely governed by such things as the output of cars, activity in the steel industry, and level of the bank rate. These are among the major factors which determine the influence of Man on the present-day evolution of our soil.

When a soil scientist studies the influence of a forest on soil formation he pays most attention to the influence of the tree canopy, which is the dominant living influence on the soil because it conditions the existence and activities of all the lesser living factors such as the ground flora, the fauna and the soil micro-organisms. But when a soil scientist studies the influence of Man on the soil he gives all his attention to the direct operations of the farmer and cultivator, the downtrodden ground flora of the human forest, and ignores the dominant influence exerted on the farmer's daily and secular activities by social and economic emanations from the canopy of the towns. My theme will be mainly the influence of towns on the secular evolution of the soil.

In embryonic societies, before towns exist, nearly everybody is engaged in food production, agriculture is of the self-sufficient type, and there is no incentive to increase soil fertility, though there is a need for the whole community to organise itself so that a minimum level of soil fertility is maintained. This need is the basis of the tribal organisation of many primitive societies living by nomadic, shifting cultivation which allows the soil to rest and recuperate between short periods of cultivation. It was also the basis of the three-field system of communal agriculture which maintained soil fertility in feudal England or, rather, slowed down the inevitable soil exhaustion that had to accompany social evolution. In these predominantly agricultural stages of human societies Man is a consumer of soil fertility. He cannot help it, any more than a young forest can help taking more out of the soil than it gives back; he cannot help it even when he is armed with all the wisdom which past experience and twentieth-century science can give him—because it is part of the nature of economic Man.

It seems also to be the nature of part of economic Man to congregate in towns at a certain stage of his social development, and to abandon agriculture for more profitable pursuits. The growth of towns has a powerful effect on soil evolution. Towns create far more, and more concentrated, wealth than agriculture can create, a rising standard of living and a greater demand for the produce of the soil. A small, but very significant, fraction of this town-made wealth flows back into the country, and the towns' demands for food, clothing and, nowadays, the agricultural raw materials of industry make it profitable for farmers to produce as much as they can

[1] Address delivered at the Sheffield Meeting of the British Association on Thursday, August 30, 1956.

from their land. To begin with, this results in an accelerated exhaustion of the soil, but if the towns continue to grow in size and prosperity a stage is reached—and has been reached in every successful civilisation—when it pays the farmers to intensify production, to increase output per acre and, therefore, to raise soil fertility. If it pays Man to increase soil fertility, he does it. That, I think, is the basic natural law governing the growth and survival of civilisation.

A good example of the initial fertility-destroying and subsequent fertility-making influence of towns is afforded by the recent history of the United States. The drain on soil fertility to satisfy the demands of British towns for cheap food in the last century was one cause of the terrifying soil erosion which has afflicted the United States. But very recently a small part of the immense wealth produced by American industry has begun to flow back into the soil. Farmers are finding that it pays to conserve their soil and to raise its fertility. Soil fertility, measured by crop yields, is rising more rapidly in the United States than in any other part of the world.

Towns increase a country's soil fertility by enabling farmers to afford to put more into the soil than they take out of it. Fertility cannot be increased merely by getting the soil to take in its own washing, that is, by self-contained or self-sufficient farming which, at best, returns to the soil only a part of what is removed from it. The fertility-producing farmer must be able to buy, or otherwise procure, fertility from outside and he must have a continuing economic incentive to do so. There are various ways in which farmers can acquire money and various forms in which they can buy soil fertility (by which I mean anything or any measure that will increase yields); but, in general, farming Man can earn enough not only to pay for his necessities and luxuries, but also to improve his land in the hope of further gain, only by selling to a stable and wealthy market—a town which produces many times more real wealth per acre than the best soil can. In this industrial age enough wealth is being produced in the towns and cities of the world to fertilize very large areas of food-producing land. Most of the people in the cities have enough to eat; most of the 60 per cent. of the world's population that are underfed are producers of food.

I once saw two soil maps of the United States, one showing the distribution of 'natural soil fertility' judged according to the amounts of plant nutrients in unimproved soils, the other showing relative crop yields obtained in different regions. As might be expected, the distribution of plant nutrients varied with the geology and had no connexion with human geography; but the highest-yielding areas were all centred round large cities, and yields tended to fall off with distance from urban centres. This is a well-known phenomenon of human geography. It is, of course, the people in the country who actually make or destroy soil fertility, but it is the people in the towns who provide the means and inducement for the others to do so.

I have so far distinguished three stages in the evolution of soil under Man. First, there is the shifting-cultivation stage when human activity has only an ephemeral effect on the soil. This stage is associated with a low density of population, and may not occur in societies living in places, like Egypt, irrigated by fertility-producing water. Secondly, as and when population increases, permanent settlement occurs and soil-exhausting agriculture is practised because society has few other sources of wealth than the soil to draw on. Society tends to develop a structure which prevents too rapid an exhaustion of the soil. This we may call the soil-exhausting stage. Thirdly, as the population increases further it congregates into towns, reducing the pressure on overworked, unimproved land, but gradually increasing the demand for its produce. Towns produce wealth from other sources than the soil that enables them to pay for their demands and makes it profitable for farmers to satisfy them by investing money in soil fertility. We may call this the soil-conserving or fertility-producing stage. Society becomes urbanised and largely loses interest in agriculture, but wealth continues to flow from the towns into the

soil. A state of equilibrium may be reached when the input of soil fertility by the towns is balanced by the output in rich harvests.

What happens subsequently is not clear. We have examples of all these three stages of social and soil evolution in the world at the present time, and we may have examples of a later stage of soil evolution under Man in the over-populated, because under-urbanised, regions of south-east Asia where nearly half the people in the world live. We do not know whether yields in India and China were once higher than they are today when they are much too low to support, except in dire poverty, the mainly agricultural populations; but whereas yields in all industrialised countries have increased markedly within the last fifty years and are still increasing, they have not increased in India and China. In both countries, however, the present governments are aware of the importance of industrialisation and getting people off the land as a means of raising the standard of living, which would lead to some improvement in soil fertility.

The different stages of soil evolution under Man are not, of course, distinct. They merge into one another, as do the corresponding stages of social evolution, and it is quite possible for all three (or more) stages to be apparent in one country at the same time—as, for example, in modern Ceylon, where shifting cultivation, soil-exhausting subsistence agriculture and soil-conserving commercial agriculture are operating simultaneously. Western Europe is the only large area of the world that is at the climax of soil evolution; much of the rest is so young in human history that it is still in the soil-exhausting stage, a fact which affords an adequate ecological reason for the present, world-wide prevalence of soil erosion. The soil-exhausting stage will pass, and one factor which is accelerating its passing is the widely felt fear that it may not pass.

A glance at the past and present histories of Man in different parts of the world will show that they all conform to the same general pattern in relation to the soil.

England

The history of England affords an excellent illustration of the way in which soils have evolved under human society from their original forest-made condition of quite low fertility to their present man-made condition of very high fertility. Parallel with this soil evolution occurred a social evolution from a tribal to a feudal to a highly industrialised capitalistic society. In these parallel evolutions the outstanding influence on soil fertility was the growth of towns.

The first people to clear the English primeval forest were probably shifting cultivators. Then, gradually, an invariable system of settled agriculture developed, of which the most essential feature was the resting fallow. This 'three-field system' was a characteristic of the Feudal Age. The land was worked according to a fixed set of rules, to prevent the otherwise rapid exhaustion of the land and the breakdown of the community. The rules not only checked soil exhaustion, but also prevented soil improvement.

The fallow, however, did not completely prevent soil exhaustion, and by the time the feudal period was coming to an end many of the open fields were getting into a bad state with increasing weediness and falling yields. As is well known, the early commerce of this country was based on wool, and the rise of the wool trade gave a great impetus to the enclosure of common land which, after enclosure, was almost invariably put into pasture for sheep. Grass is the best soil-improver known; indeed, it is noteworthy today that wherever soil improvement is being planned, from the Poles to the Equator, first reliance is placed on grass. At the time of the Tudor enclosures, at the end of the exhaustive stage of soil evolution, it was pressure from commercial interests, and against the will of the great majority of farmers, that gave the soil its first dose of fertility-producing medicine. Later, great improvements, which would have been impossible on unenclosed land, were

effected in pastoral and arable farming, mainly with capital earned in the towns. Investment in soil fertility was profitable because the towns provided a market for all that the soil could be made to produce.

Large-scale investment in soil fertility of money earned in commerce and industry continued until about ninety years ago with immense benefits to both farmers and land. Then the opening-up of the New World brought near-disaster to British agriculture, and offered greater attractions than did British land for the surplus wealth of the towns.

However, the subsequent neglect of British agriculture, which lasted until 1940, had little effect on the inherent fertility of the soils because so much land went back to grass, which gave the soil a rest. If arable farming had been maintained at the 1870 level with insufficient capital investment, the loss of inherent soil fertility might have had serious consequences in the two world wars. At the present time the crying need of the soil is for capital which can only be provided in sufficient quantity by the products of industry. It is becoming evident that, in future, Britain will be unable to rely to the same extent as formerly on buying unlimited food from abroad, so more of the wealth of the towns may again be diverted into the soil. Already the State pours money into the land on a vast scale; the level of soil fertility—crop yields—would fall immediately if the State ceased to do so.

North America

In North America the soil is going through a similar sequence of evolutionary stages under the influence of Man. Social development has been telescoped into a much shorter space of time than was the case in Europe. Most people would say that industrial progress has advanced further in America than in Europe, but it is of very recent date and the beneficent effects of American industrialism on the soil are only now beginning to be discernible.

At first there was a period of 'shifting cultivation' as the frontier was pushed westward. The land was skimmed of its fertility and then abandoned or passed on to another who continued the skimming process. The greater part of the habitable land was occupied within a century. Then followed a period of soil-exhausting agriculture when the unimproved soils were bled not only to keep their owners alive, but also to feed the teeming urban populations of Europe and thereby to provide some capital for founding American industry. The land got back little for what it gave, but in the mushrooming cities seeds were being sown which would bring forth a rich harvest of soil fertility.

The disastrous effects on unfertilised American soil of huge exports of food, mainly to Europe, are very evident at the present time in the widespread occurrence of soil erosion, a disease from which many other parts of the world are also suffering. Food exports, of course, were only one of many causes of the rapid exhaustion of American soils that in its turn was the immediate cause of soil erosion by the physical breakdown of soil structure. That all this erosion should have happened is usually regarded as unfortunate, sometimes as tragic, and occasionally as sinful. Taking a global view of agriculture, soil erosion is certainly a phenomenon of tremendous significance today. It has been described as a symptom of maladjustment between society and the soil, but I regard it, rather, as a symptom of a normal stage of the evolution of soil under Man's control. Human society destroys soil fertility before it begins to create it, and there is nothing society can do about it until it has created a great surplus of wealth, over and above what the land can produce, with which to fertilise the soil.

Unlike Europe, North America has not evolved a cast-iron social system to check the outflow of fertility from the soil. Events have moved too quickly. But in the 1930's the soil-conservation-district movement was started in the United States, by which the farmers of a district voluntarily organised themselves, with Federal

and State backing, to farm according to established soil-conservation practices. The movement spread with astonishing rapidity, and today most of the farm land is included in soil-conservation districts. In many districts good intentions are more evident than soil conservation, but that the movement should have swept the whole country in less than twenty years is most significant. The much greater effect on soil fertility of a phenomenal increase in industrial production has to some extent masked the direct effects of soil-conservation measures.

Although the event is still too recent for us to be certain about its significance, the economic depression of the 1930's may have been the turning point in the evolution of American agriculture from soil-exhausting to soil-conserving. During the depression millions of acres of overworked land got a rest, and the virtues of grass as a protector of the soil from erosion and as a renovator of soil fertility became clear to all. As in our first agricultural revolution, the farmers did not like having to change their traditional ways, but they could not stand up to the harsh economics of the time, any more than our open-field farmers could resist the powers of enclosure. When the second world war came, food production was enormously increased, as it had been in the first war, but this time fairly adequate precautions were taken to protect the land from erosion, and soil fertility was not used up—indeed, it was increased by the greatly expanded use of fertilisers and other applications of science and technology. Since the war, crop yields have continued to rise, and now average about 35 per cent above pre-war. Farmers have had money to spend and to spare, and some of it has found profitable investment in soil fertility. Boom conditions, however, do not last for ever. America is now producing more from its land than it can dispose of. What that portends for the future I do not know, but it suggests that American economy and soil are still far from a balanced equilibrium. The soil-conservation stage has a long way to go.

USSR

Data on the progress of agriculture in the Soviet Union are unreliable, but there is no evidence whatever of such great advances in yields and intensity of production as have recently occurred in North America. In Russia the towns do not provide surplus capital to fertilise the land; on the contrary, the land is starved of capital to feed the expansion of industry, as happened in the United States until a few decades ago. Russia is still in the soil-exhausting phase of economic development—indeed, in some respects it is still in the shifting-cultivation phase. If the industrial revolution is carried through successfully in Russia, however, the land should ultimately get some of the surplus wealth of industry in the form of capital investment and applied science, and the normal effects of industrialisation on soil fertility should then appear. Russian soil science is remarkable in two ways. It is twenty-five years ahead of the rest of the world in its conceptions and twenty-five years behind in its application. The limiting factor to greater productivity is not lack of knowledge of the soil, but lack of capital as a fertiliser. To this might be added the apparent absence of all incentive to the collective farmer to improve the land. The present trend in Russia is towards the supersession of the collective farm by the State-owned, factory-operated farm. Collective landownership, during the short time it has operated, has failed to increase soil fertility. It is quite possible that State ownership, which is in some ways analogous to the large-scale individual ownership which played such an important part in promoting soil fertility in England, may have similar effects in Russia. To the western mind the much advertised project to reclaim seventy million acres of semi-arid virgin land in Central Asia for grain production seems a colossal waste of effort when so much more could be done by intensifying production on the naturally fertile and more accessible black earths of the Ukraine, but it must be remembered that so far the influence of Man on the soils of the Soviet Union as a whole has been very small,

and parts of that vast country are still in the shifting-cultivation stage. There is still the urge to people the empty spaces, that appears again and again, and not only in Russia, in schemes to reclaim deserts or to settle the Arctic, and reflects the inborn longing of Man to be master of all he surveys.

One must recognise, too, that the Chinese communist revolution, with its emphasis on industrialisation, may bring new life to China's worn-out soils, many of which seem to be in the last stages of decline after some thousands of years under Man's control. But the revolution has scarcely started yet.

South Africa and Australia

These two large countries are taken together not because of any similarity in their agriculture or soils, but because both are at the same critical stage of soil evolution. In both, soil exhaustion and erosion have been very severe and have caused the utmost alarm to farmers, financiers and politicians. Indeed, the late General Smuts once said that soil erosion was bigger than politics—which meant something in South Africa!

Since the last war, however, a remarkable change in outlook has come over both countries. Immense progress, for so short a time, has been made in the reorganisation of agriculture on a soil-conservation basis, particularly by the establishment of soil-conservation districts based on the American model, and by the intensification of agriculture and the introduction of ley farming. In both countries, too, agriculture has ceased to be the main occupation of the inhabitants. In Australia three-quarters of the whole population is now urban. In South Africa heavy industry produces more wealth than either mining or agriculture. Both countries have just reached the stage where the wealth of the towns can begin to fertilise the soil.

The voluntary communal control of soil erosion by means of soil-conservation districts, that has taken such firm root in America, Australia, South Africa and also on European land in Rhodesia seems to be the modern equivalent of the communal farming rules enforced to check soil exhaustion throughout feudal Europe. Land-use regulations, made to ensure the maintenance of soil fertility, are enforcible by a district's own laws, as the fallow was enforcible by manorial law. The old three-field system, however, merely prevented soil exhaustion from going too fast. The soil-conservation district aims not only to prevent soil erosion, but also to build up fertility—which was impossible under the three-field system. The soil-conservation district may well turn out to be the characteristic not only of the final stage of the soil-exhausting phase in these rapidly growing nations, but also of the emerging fertility-producing phase. It was originally devised to check the precipitate exhaustion of the soil that, in the previous absence of any social control, was getting out of hand, but it is now being used everywhere to build up soil fertility. The soil-exhausting phase is merging into the fertility-producing phase.

In South Africa, in particular, the soil-conservation-district movement has swept through the country within the last few years only. A sudden impetus has been given to soil conservation, the results of which have not had time to appear, but there can be no doubt about the impetus which, again, may not last. It does seem, however, that the great progress and prosperity of South African industry are convincing farmers that it will pay them to invest in soil fertility, for example, by adoption of ley farming, by applying sulphate of ammonia to grassland in order to build up the soil's humus content, and other measures whose lasting efficacy cannot be known for many years. The significant fact is that the spirit of soil conservation is abroad, inspired by the money flowing from South Africa's young industries.

Australian pastoral and arable farming is also tending to become fertility-producing, though, as in South Africa, the revolution, if it is one, has scarcely begun.

The creation of more fertility than was present originally in Australia's soils has been made posssible by using superphosphate to grow wheat and clover. Australian soils are among the oldest in the world, and were poor in the two essential plant nutrients, phosphorus and nitrogen, even before soil-exhausting farming began with the arrival of the white man. Wheat and wool have since removed much of the remaining nutrients. Deficiency of phosphate is widespread in both agricultural and pastoral land, and trace-element deficiencies are common. A general advance in Australian soil fertility can only be achieved by overcoming these deficiencies. There is also a deficiency of water that is more difficult to overcome, but Australia has a long way to go before water becomes the final limiting factor.

By applying superphosphate to the—to European eyes—miserable Australian pastures which, nevertheless, produce the finest wool in the world, dense crops of subterranean clover can be grown that enrich the soil with nitrogen, double or treble its carrying capacity and provide humus for more intensive arable farming. By such simple means, reminiscent of the introduction of clover into English farming, there are almost limitless possibilities for increasing the fertility of Australian soils.

Superphosphate, subterranean clover and a few trace elements have the power to make at least much of southern Australia fertility-producing. But the existence of the means is not enough to effect the revolution. The high price of wool, that resulted from the Korean war, gave a great fillip to soil improvement, but will not last for ever. The Australian people, however, are already three-quarters urban and are developing secondary industries which should produce a surplus of wealth with which to fertilise the soil. Australians occupy a huge continent and are concentrated mainly in five large cities. It remains to be seen how far the fertilising influence of these five widely separated cities will spread into the outback, most of which is still in the shifting-cultivation stage.

Tropical Lands

In the mostly thinly populated areas within tropical latitudes, Man has seldom succeeded in ousting the plant world from its dominant position in the soil's economy. The Indian sub-continent is the best existing example of permanent tropical agriculture that has continued for centuries. It is also one of the most densely populated of tropical countries. As elsewhere in the tropics, the basis of this permanent agriculture has been paddy cultivation in which flooding suffices to maintain plant nutrients in the soil at a level adequate for at least subsistence production of rice. The example of other countries, like Japan and Australia, shows that rice yields could be greatly increased in India by fertilisers, mechanisation, use of high-yielding varieties, etc., and there should be no difficulty in providing all the people of India with adequate food from her soil, if the wealth to fertilise the soil were there—which, of course, it is not. There is far too high a proportion of the people on the land for its efficient utilisation, and they are too poor to fertilise it. The rapid increase in India's rural population within the last century seems to have accelerated soil exhaustion, at least so far as soil erosion is symptomatic of it. There has been no increase in average crop yields during this century. This may indicate the normal exhaustion phase of soil evolution under Man, to be followed by a conservation phase when the country has been urbanised and enriched by industry, or it may represent a later phase in which society is too old to adapt itself to the creation of soil fertility. The Indian government is exerting every effort towards industrialisation, wherein undoubtedly lies the main hope for the future fertility of Indian soil.

No other well-tried system of settled agriculture except paddy-rice growing is known that will at least maintain, if not increase, the fertility of tropical soil. Rice is the almost universal basis of settled tropical agriculture, as wheat is of temperate agriculture. The minerals in the flood waters together, perhaps, with nitrogen fixed

by algae often found on paddy fields usually suffice to maintain soil fertility under continuous cultivation for hundreds or even thousands of years without needing a very complex social organisation to operate the system. Rice growing, with a little pasturage and livestock, can provide the minimum necessities of a settled tropical society. Otherwise, tropical agriculture is mainly of the shifting-cultivation type which precludes permanent settlement. A patch of land will be cleared and cultivated for two or three years, after which the available plant nutrients in the soil will have been used up, and crops will fail. The land is then abandoned for, say, ten to twenty years, during which a secondary growth of vegetation will invade the soil, restore its fertility and make possible another short period of cultivation. Shifting agriculture is essentially exhaustive, the purpose of the abandonment of cultivation being to rest the soil and restore its fertility. Such a system can only work with a very low population density.

The impact of European civilisation on the tropics has greatly accelerated, but does not seem to have altered, the normal course of soil evolution under Man. European colonists cannot live by shifting cultivation, and they have tried with some success to introduce peace and better health into their colonies. Consequently, colonial populations have recently tended to exceed the limits at which the land can be rested long enough to restore its fertility. In every tropical colony (using the term in its widest sense) shifting cultivation is breaking down, and invariably and inevitably soil-exhausting settled agriculture is taking its place. Social and soil evolution is going through the normal stage of soil-exhausting agriculture, often accompanied by catastrophic soil erosion. The wealth required to create soil fertility and, still more, the demand for a high standard of nutrition from a large, well-to-do urban proletariat are absent. Until this demand appears there will be no incentive to bury money in the soil. To the few Europeans who operate highly capitalised plantations in the tropics, however, the incentive of supplying their own urban markets, at home and abroad, is making itself felt.

We already have examples of intensive, fertility-producing agriculture in the tropics, that is basically similar to intensive European agriculture. In Southern Rhodesia a system of ley farming with large applications of nitrogen to the grass has given consistently high yields of maize, meat and milk, and has improved the condition of the soil. But it has not been operated long enough to merit the term permanent agriculture. The system is worked by a few progressive Europeans whose extra output is not sufficient to depress the price of maize. If every farmer followed suit there would be such a glut that all would be ruined—or at least would be unable to buy the necessary fertilisers. Under present conditions there would be insufficient demand for the produce. On the other hand, if all the people in the towns could afford to live well their demands would tend to raise the price of maize, and some of the money they spent would flow back into the soil. All—and it is a big all—that Rhodesian soil needs to make it fertile is more and richer townspeople. Until it gets them it will have to put up with a good second-best—the magnificent work of its agricultural officers. I should like to pay a tribute to this handful of key men who, throughout our tropical Empire, are smoothing out the agonies of the violent agricultural revolution which has followed the break-up of shifting cultivation, and are preparing the ground for the next, more prosperous stage.

Most colonial countries are now in the early soil-exhausting stage of evolution, and are developing social and agricultural systems which will slow down the loss of soil fertility that is bound to occur before the peoples are numerous and wealthy enough to enrich the land. Today, in most colonies, agricultural society is being reorganised, largely by agricultural officers, on a basis of soil conservation with laws, ordinances, sanctions and subsidies to ensure at least the safety of the remaining soil. One can see social systems evolving in which it may be as difficult to mishandle the soil as it was in feudal England. In a recent flight over Africa what impressed

me most was the quite frequent appearance of that most characteristic feature of soil conservation—terraced, strip-cropped fields. It was also the most beautiful feature of the generally dismal view one gets of Africa from the air. The open fields of England might have given a balloonist a similar impression five hundred years ago.

These emerging social and agricultural systems, designed to conserve tropical soils, tend to be less flexible and more compulsive than those which are evolving in temperate regions whose inhabitants are politically and socially more advanced. They may become as unadaptable to purposes of soil improvement, as distinct from soil conservation, as was the rigid three-field system of England. We are acquiring the knowledge to make tropical soils fertile, but there are still lacking millions of people in towns producing non-agricultural wealth, the best fertiliser soils can have.

Conclusion

Throughout history the picture of Man in his relation to the soil has had certain common features: his first struggle to adjust himself to the existing balance of Nature either by adopting shifting cultivation in forest lands or by nomadism in grass lands; then, with increasing population, upsetting the balance of Nature by the practice of settled, subsistence agriculture with social checks on the unavoidable exhaustion of the soil; then the concentration of the growing population into towns, the creation of new wealth in manufacturing, commerce and the arts, a rise in the urban standard of living, a demand for more of the necessities of life, an overflow of wealth into the soil, and the creation of new fertility to satisfy the towns' demands; finally, the re-establishment of a biotic balance when the inflow of soil fertility is balanced by the outflow. So long as most of the population is urban there is no apparent upper limit to the number of people who can live in a region or country without exhausting its soil; but the present-day condition of south-east Asia suggests that a relatively low total population density can be a heavy burden on the soil when most of the people live on the land. By contrast, the countries showing the highest average soil fertility are the most densely populated and highly industrialised—Britain, Germany, Holland, Belgium, Japan—and agricultural Denmark, the exception to prove the rule.

Today, as a result of the rapid opening-up and development of a large part of the habitable land within the last century, most of the world is in the soil-exhausting phase, a fact which, unless viewed in ecological perspective, may lead to a certain loss of faith in the future of mankind. But it is a passing phase, which seems alarming only because it is happening over such large areas at the same time. Already we can see signs in some rich new countries that the soil-conserving phase is approaching. Will the world of one hundred years hence be able to feed the 6,000 million people who will then be in it? The answer is yes, provided most of them live in towns and produce enough wealth to pay for the food they need. If they offer enough money for their food, the food will be produced. As every farmer knows, it pays to fertilise when the market is good. That may, perhaps, be regarded as an over-simplification of the phenomena of civilisation, nevertheless it explains quite a lot of them.

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The British Association remains unique as an independent institution of national reputation, which brings almost the whole range of sciences within its scope and opens its membership to all who are interested in the progress of science. Founded in 1831 to convince an indifferent public and Government that science was important and worth backing, the Association—as its history bears witness—succeeded magnificently in its original purposes. Today science is of paramount importance to the Country and one of the Association's main tasks is to promote a better understanding of the significance for industry of scientific research and its impact, through its applications, on society as a whole.